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ART. XXXII.—*The Primitive Diversity and number of Animals in Geological Times;* by L. AGASSIZ.

THERE is a view generally entertained by naturalists and geologists that genera and species of animals and plants are greatly more numerous at the present age of the world than in any previous geological period. This seems to me an entire misconception of the character and diversity of the fossils which have been discovered in the different geological formations, and to rest upon estimates which are not made within the same limits, and with the same standard. Whenever a comparison of the diversity and number of fossils of any geological period has been made with those of the living animals and plants belonging to the same classes and families, it has been done under the tacit assumption which seems to me entirely unjustifiable, that the fossils formerly inhabiting our globe are known to the same extent as the animals which live at present upon its surface; while it should be well understood that however accurate our knowledge of fossils may be, it has been restricted, for each geological formation, to a few circumscribed areas. Comparisons of fossils with the living animals ought, therefore, to be limited to geographical districts corresponding in extent to those in which the fossils occur; or, more properly, a fossil fauna with all its local peculiarities ought to be compared with a *corresponding* fauna of the present period, and not with *all* the animals of the same class living at present *upon the whole surface* of the globe. And when this is done

with sufficient care and proper allowance is made for the limited time during which investigations of fossils have been traced compared with that which has been almost everywhere devoted to the closer study of living animals, it will be seen that the number and diversity of species peculiar to each special fossil fauna is, in most instances, equal to those found to characterize zoological provinces of similar boundaries, at the present day. And this may be said of the fossil faunæ of all ages. In many instances the result is even quite the reverse of what is generally supposed to be the fact, for there are distinct fossil faunæ which have yielded much larger numbers of species, presenting a greater variety of types than any corresponding fauna in the present age. Some examples will justify this perhaps unexpected statement.

The number of species of shells which are found living along the shores of Europe, does not exceed six hundred. About six hundred species is again the number assigned to the whole basin of the Mediterranean, including both the European and African coasts. Now the most superficial comparison between them and the fossil species which occur in the lower tertiary beds in the vicinity of Paris, shows the latter to exceed twice that number; there are indeed twelve hundred species of fossil shells now known from the eocene beds in the immediate vicinity of Paris, affording, at once, a very striking evidence of the greater diversity and greater number of species of that geological period when compared even with those of a wider geographical area at the present day.

If it be objected that the variety of forms which occur in tropical faunæ is greater than that which we observe on the shores of our temperate regions, and that the temperature of the tertiary period having been warmer we may expect a larger number of fossil species from those deposits, I would only refer to local enumerations of marine shells from several tropical regions, to sustain my assertion that the number of fossil shells of the eocene beds of the *immediate vicinity of Paris*, is much greater than that of any local fauna of the present period, even within the tropics. A catalogue of not quite three hundred species of shells given by Dufo as occurring around the Sechelles Islands, the extent of which may fairly be compared with that of the lower tertiary beds around Paris, will suffice to show, that in a tropical local fauna the number of species known to exist in the present day is far inferior to the number of species known to have occurred during the deposition of the lower tertiary beds in the vicinity of Paris. Another catalogue by Sganzin, of the shells found about Mauritius, Bourbon and Madagascar, gives also less than 300 species for that extensive range of seas surrounding those islands. Let us further compare the results of the investigations of the shells of the Red Sea by Hemprich, Ehrenberg and Rüppel, and there

again we find a smaller number, and a more limited variety of types than are found in the tertiary of Paris; for the whole basin of the Red Sea has thus far yielded only 400 species of shells. Let us finally take the most accurate survey of this kind we have of any shore, that of Panama by Prof. Adams, extending over 50° of latitude, 28° N. of the equator, and 22° S. of it, including the most favorable localities for the growth of shells in the Pacific under the tropics, and yet we shall find his list exceeding but little the number of 500 species. In this instance again we find that the advantage in number and variety is in favor of the tertiary period, and not of the present age. If a different result has been obtained by the estimates made before this, it is owing to the circumstance, that the *fossils known from a few localities within narrow geographical limits* were compared with the *living species known to occur upon the whole surface of the globe.* But let us trace these comparisons through other geological periods, with reference to other classes also, and we shall find in every instance, similar results. The tertiary fossils of Bordeaux, though less numerous in species than those of the eocene in the vicinity of Paris, will compare with any local fauna of the present period as favorably for variety and number of species as those of the lower tertiaries. This may be said, with the same certainty, of the tertiary shells of the Sub-appennine Hills, or of those of the English Crag of which we now possess a very complete list.

If from the tertiary periods we pass down to the cretaceous, do we not find in the deposits of Mæstricht, or in those of the age of the white chalk, a number and variety of shells as great as that which may be found on any shore or in any circumscribed marine basin of an extent at all comparable with that of the cretaceous beds within similar limits? Do we not find in the lower cretaceous strata such as the green sand or the Neocomien, other assemblages of the remains of Mollusks, which, in number and variety, are not inferior to those of the white chalk? The oolitic series, again, will stand a similar comparison quite as well. We need not even take the whole group of those deposits, but consider each subdivision of the Jurassic period by itself, and still we find in every one, local faunæ of Mollusks, assuming of course, a different character from those of the cretaceous or tertiary, but nevertheless sufficiently diversified to admit of an estimate, as advantageous, with respect to the points under consideration, and to the local faunæ of the present day as to the cretaceous assemblages of fossils, or those of the tertiary period. Of course, in accordance with the peculiar character of the age, different families prevail in these different periods; the Cephalopoda are extremely numerous and surprisingly diversified during the cretaceous and oolitic periods; while they dwindle down to

a few representatives in the tertiaries, and so with other families. The shells found in the deposits of the new red sandstone period, of the coal period, and of the still earlier ages, are perhaps less numerous on the whole, though they can hardly be said to be less diversified; for, the extinct forms which occur among them, are quite an equivalent to the variety of their families which have lived during more recent periods; and the daily increase of the species found in the different palæozoic deposits shows that, even in point of numbers those ancient faunæ may, even in the present state of our knowledge, be compared with local faunæ of similar extent at the present day.

Desirous of making the most accurate comparison possible between the *subdivisions* of the paleozoic formations of the state of New York with *local faunæ of similar extent* in the present seas, I have requested Professor J. Hall to furnish me with summary indications respecting the results of his extensive investigations in this field, and I have obtained from him the following statement:

"I regard the *Potsdam and Calciferous Sandstone* as disconnected with the groups above, forming of themselves with their fauna (not yet well known in this country) a distinct geological period. The entire number of species thus far known in these rocks, admitting all of Owen's species, is however only twenty-six."

"The *Chazy limestone* has 45 species restricted to itself, and one other species which is also known in the Black River Limestone. The *Birdseye limestone* has 19 species restricted to itself and two others which pass upwards. The *Black River limestone* has 13 species restricted to itself, and one common to it, and the Chazy limestone, one common to it and Birdseye, and one common to it and the Trenton, and one other which is common to the beds below and above, extending into the Hudson River group," making together 81 species for these three sets of beds.

"The *Trenton limestone* has 188 species restricted to itself, and 30 species passing upwards into the Hudson River group. The entire number of species known as occurring in the Trenton limestone, including those which occur in rocks above and below is about 230. This statement includes some species discovered since the publication of the 1st volume of the Palæontology of New York, and which would make the restricted species about 200."

"The *Hudson River group*, including Utica slate, has about 60 restricted species, besides those which are common to it and the rocks below, making altogether about 100 species."

"You will observe that the development of life at the Trenton period, has been far the most marked, though it is true that this

formation is much thicker than either of the preceding limestones, the Chazy being the thickest, and the Black River the thinnest of the three below the Trenton."

"In that portion of the upper Silurian period included in the 2d vol. of the Palæontology of New York, the fossils of the Medina Sandstone, Clinton group, Niagara and Onondaga Salt groups, amount to 341. *Medina and Clinton groups* 123 species. *Niagara and Onondaga Salt group*, 218 species."

"The Medina Sandstone and arenaceous beds of the Clinton group contain 50 species, leaving for the calcareous beds of the Clinton group 73 species, which, added to the 218 species of the Niagara and Onondaga Salt groups, give 291 species as the total number of species of the calcareous beds of these groups. The Niagara is here the more important period, and though not thicker than either of the others, contains about 200 species restricted to itself. Of the Niagara group 67 species are Corals and Bryozoa. Of the 73 species from the calcareous beds of the Clinton group, 19 are Corals and Bryozoa."

"In the *lower Helderberg group*, including the Water lime, Pentamerus limestone, Delthyris Shaly limetone, and upper Pentamerus limestone, I expect to describe about 200 species, exclusive of Corals and Bryozoa, of which I know already about fifty species."

"The *Oriskany Sandstone* may contain about 60 species of fossils altogether, perhaps less."

"In the *upper Helderberg group*, which is the next great Calcareous formation, I anticipate a less number of species except Corals and Bryozoa, of which there are more than 100 species in New York and the western localities. Of all that is yet known in these limestones besides Corals and Bryozoa, it would be unsafe for me to estimate more than 100 species."

"From the *Hamilton*, *Portage* and *Chemung groups* I anticipate at least 300 species within New York, and I shall not be surprised if more complete investigations produce double that number in New York and the West."

"The number of species given here I regard as only approximate. I hope this general statement may meet your present requirement, but I regret that I cannot now give you more definite information, particularly regarding the Upper Helderberg. I give you from this and the higher groups an estimate based on the species known to me at the present time; but my final investigations always reveal a greater number than I anticipate."

These statements of Professor Hall place already each of the principal group of rocks of the state of New York in the category of distinct independent successive faunæ, equivalent each to as many local faunæ of the present period, for we may repeat that the fauna of the Sechelles contains only 258 species, and that of

Mauritius, Bourbon and Madagascar, 275. Nay, upon 3000 miles of coast along the western shores of the American continent, within the tropics only twice the number of living species have been obtained as occur respectively in each successive greater subdivision of the paleozoic system within the narrow limits of the state of New York only. (See above the results of Professor Adams's investigations upon the coast of Panama.)

It is a most unexpected and very significant coincidence that the late admirable investigations of Elie de Beaumont upon the mountain systems, have led him to the recognition of nearly ten times as many periods of great disturbance in the physical constitution of the earth's surface, as he himself knew twenty-five years ago, each attended by the upheaval of as many mountain chains, differing in their main direction. The investigations of palæontologists having an entirely different character, and founded upon facts which until recently have apparently had only a remote connection with the other series of phenomena, have nevertheless brought them at about the same time to like conclusions respecting animal life, showing that the periods of disappearance and renovation of organized beings upon earth, have been much more frequent than could be supposed even ten years ago, each set having probably been characteristic of one of those long periods of comparative rest, intervening between two great successive geological cataclysms.

What is true of Mollusca, may be said of all other classes. Among Radiata, are not the coral reefs of the palæozoic ages as rich in species as any coral reef of the Pacific? Let us even compare the most extensive list of corals yet given as belonging to any circumscribed locality,—those of the Red Sea as described by Ehrenberg,—those of the Feejee Islands as described by Prof. J. D. Dana,—and let us inquire whether the palæozoic rocks of the state of New York do not show as great a variety and as large a number of species in their successive reefs. Again, the coral reefs of the oolitic period in Normandy, or in the Jura of Switzerland, and the Alp of Wurtemberg, have they not increased our lists of fossils as largely and introduced into our zoological works as various forms as are known from any of the most diversified coral regions in the world at the present day?

Passing from the corals to the Echinoderms, the question may be reversed, and it may be fairly asked whether there is any sea shore extending over tens and tens of degrees of longitude and latitude even under the tropics which has yielded as large a number of those Radiata, as occur in almost any of the geological formations? The number of Crinoids found in the single set of beds known under the name of Niagara limestone, equals the whole number of Echinoderms found around all the coast of the United States. The Crinoids, Echini, and Star-fishes of the

oolitic period, or any of the subdivisions of that formation, surpass the number of species of that class which may be gathered around the coast of entire continents in the present day. The diversity of forms of these animals comparing them with those of the cretaceous periods, is equally great, though the Crinoids begin to diminish in number. But the variety of Spatangoids and Clypeastroids which come into play, compensate largely for the diminution of the family of Crinoids.

The type of Articulata may seem, in the present condition of our knowledge, to form an unanswerable objection to the broad statement I have made above, for the hundred thousands of insects which are known in the present creation will hardly allow a comparison with the fossils. But let us examine upon the principles by which we have been guided in the preceding computations, what is the true state of things respecting the occurrence of Articulata in former geological periods. We can, of course, hardly expect to find worms well preserved in geological formations, on account of the softness of their body, which will scarcely allow of preservation to a greater degree than Medusæ. But a few instances in which impressions of these animals have been found justifies the assertion that they existed as well in former periods as now. The impressions of Medusæ found in the lithographic limestone of Solenhofen, which are preserved in the Museum of Carlsruhe, not only carry back the existence of this class to the Jurassic period, but justify the question whether a large number of the fossil polypi from older periods, which have been described as belonging to that class, are not in reality nurses of Medusæ similar to the Campanulariæ, and Sertulariæ of the present day, which are now known to be no Polyps, but one of the alternate generations of Medusæ. And as for the worms, we find in each geological formation, from the oldest to the most recent, fossil Serpulæ, or similar solid cases of worms in as large numbers as we find these animals any where at the present day. And where the existence of Serpulæ is established by such unquestionable evidence as that of their calcareous cases, are we not justified in the inference that those entirely naked worms which are found every where existing with Serpulæ, had also their corresponding representatives during former geological periods?

With the class of Crustacea the difficulty in the comparison is already less; for, in the tertiary beds of Sheppy there have been found a variety of lobsters, shrimps and crabs, which would favorably compare with the crab fauna of any limited shore in the present day; and I doubt very much whether such a variety of Crustacea could be collected any where on a shore of equal extent to that of the white chalk of Sussex, as Dr. Mandell has uncovered in the vicinity of Lewes. For a comparison of the Crustacea of the oolitic period, I would only refer the skeptic to the

monograph of the Crustacea of Solenhofen by Count Münster, who has figured from that single locality more species than are known in the whole basin of the Mediterranean, excluding the minute species which have not yet been sought for among the fossils.

In earlier geological ages, during the deposition of the coal and other palæozoic rocks, the class of Crustacea presents a very different character. The gigantic Entomostraca and the extinct family of Trilobites take the place of the lobsters and crabs of later periods. But palæontological works illustrating the fossils of Sweden, Russia, Bohemia, England and France, have made us acquainted with as great a variety of species of those families as are found of the later representatives of the class in more modern deposits. So that among Articulata the class of Crustacea can be said to have been, at all periods, as largely represented, and to have shown as great a variety of forms as occur any where within similar limits in the present time.

The carcinological fauna of the whole Indian Ocean scarcely exceeds in variety or number of species that of Bohemia alone, as it is now known by the admirable investigations of Mr. de Barande.

From their minuteness and general structure, Insects might be excepted in such a comparison without affording a sufficient argument against the view I have taken of the subject, even if insects had nowhere been found in large numbers in a fossil state. For it must be plain that their preservation requires more favorable circumstances than the preservation of other animals more largely provided with solid parts. But though the fossil insects have not been sufficiently investigated in all geological formations, have we not several examples which show that in some geological periods, at least, they were as numerous as in the present day? The beautiful Monograph of Behrens, of the insects which occur in Amber, shows how varied these animals were during the period of the formation of that gum, and the unparalleled investigations of Professor Oswald Heer upon the insects of Oeningen and Radeboy have furnished us with means of comparisons which show that during the deposition of the Molasse of Switzerland, the insects were as numerous and as diversified there as they are any where in our day, within similar boundaries. And the fragmentary information which we already possess upon the insects of Aix in Provence, and those of Oeningen will justify the expectation that insects will finally be found very numerous in all the geological periods from that of the carboniferous deposit to the present day; that is to say, ever since terrestrial vegetation has had an extensive development. The discoveries by Hugh Miller of true trees in the old red sandstone will justify the prophecy that insects will be found, some day or other, even among palæozoic rocks older than the coal period.

But what of the Vertebrata? Is there not evidence, that, at the present day, they are more diversified and more numerous? Here again I answer, decidedly, No; granting only that there are periods during which the higher classes of these types did not exist, and that therefore, as a type, the vertebrata of the present day are more diversified; but the individual classes, from the time of their appearance have been in each former period, as numerous and even as various as they are at present.

Let us apply to these the same measure which we have applied to the Radiata, Mollusca, and Articulata, to justify this assertion, which seems so completely at variance with our knowledge of fossil vertebrata. Fishes occur, as is well known, in all geological formations. But should we compare the fossil fishes of each geological period as they are known from a few localities, with the whole number of fishes which exist all over the world in our day? It would be as unphilosophical as it would be inconsistent with our knowledge of the geographical distribution of animals. Like all other living beings, fishes are located within definite boundaries, and it will be but fair to compare the fossil species of a given locality with the special Ichthyological faunæ which occur in different oceans, or in different fresh-water basins. Now with this rule we may institute a comparison of the fossil fishes with the living ones, with reference to their number as well as to their variety.

The number of species of fossil fishes known at present from the tertiary deposits, in a single spot, upon the Island of Sheppy, is greater than the number of fishes which have been gathered around the coast of any of the islands of the Pacific Ocean, as far as we know the local Ichthyological faunæ of those regions; it is as great, nearly, as the whole number of fishes known from the shores of Great Britain. The same may be said of the fishes of Mount Bolca, or of Mount Lebanon, or of those of the white chalk of England, or of those of Solenhofen, or of those of the lias of Lyme Regis; and if we pass to older deposits, to the old red sandstone even,—thanks to Mr. Miller, and to the investigations of other British and Russian geologists,—do we not know from that old formation as many fishes as from any of the more recent ones, or from any circumscribed marine basin? and is not the variety which occurs among them at each period as great, though of a different character in each, as the variety which occurs at the present day? So that it can be fairly said, that at all periods, fishes have presented as great a variety of forms, and as numerous species, as under corresponding circumstances at the present day.

The class of Reptiles will allow similar conclusions, for though the giants of the class have chiefly been studied, do they not indicate an abundance, and a variety of these animals during the

upper secondary formations, as great as in any tropical region? and have we not sufficient indications among the tertiaries to be justified in expecting that they also will turn out to be more numerous than they are now known to be?

The class of Birds seems to form an exception in this view. But there seems to be particular reason why the bones of birds should be more liable to destruction and decomposition than those of other vertebrata. And whoever has traced the discoveries made recently among the fossils of this class, will certainly not insist upon a supposed scarcity of birds in former periods, but rather be inclined to admit that the limited number now known is to be ascribed to the deficiency of our knowledge rather than to a want of these animals in earlier formations, indications of their presence having been ascertained for several tertiary formations, for cretaceous deposits, and even for deposits belonging to periods older than the chalk.

Fossil Mammalia are comparatively too well known to call for many remarks, after what has been said above. Let us only remember that the number of fossil species found in Brazil alone equals the whole number of Mammalia known to live at present in that country; that the fossil Mammalia of New Holland compare already favorably with the living species of that continent; and that the locality of Montmartre alone has yielded as many large Mammalia as occur all over Europe, and the Mauvaises Terres in Nebraska as many as may be found in North America now. So that, if we grant simply that among vertebrata the diversity has been increasing with the successive introduction of their different classes, the number and diversity of these different classes at each period has been as great as it is at present.

These facts are of the utmost importance with reference to the great question of the order of succession and gradation of animals in the different geological periods. They cut away forever one of the arguments upon which the asserters of the development theory have insisted most emphatically. Before it could be granted that the great variety of types which occur at any later periods has arisen from a successive differentiation of a few still earlier types, it should be shown that in reality in former periods the types are fewer and less diversified; and we have now shown that this is so far from being the case, that in many instances the reverse is really true. I have already attempted elsewhere to show in outlines what is the real order of succession of the great types of the animal kingdom, I need not therefore repeat here what may be gathered from the diagram at the head of the Zoological Text Book I have published jointly with Dr. Gould. I shall limit myself to a few more general remarks upon the special difficulties involved in a more thorough investigation of the subject.

The study of the order of succession and gradation of the organized beings which have inhabited our globe at different periods, presents indeed difficulties of more than one kind. Unhappily these difficulties have seldom been all considered in their natural connection by those who have ventured to consider the subject in its whole extent; thus presenting certain results as general which would require various qualifications to be true. In comparing fossils of one and the same or of different geological formations, it is in reality not enough to ascertain their true geological horizon, which we may call the *chronological element* of the enquiry; it is equally important that the differences or resemblances arising from the geographical distribution over the wide expanse of the whole surface of the globe, which we may call the *topographic element* of the question, should be also considered, for it is already known that within certain limits the same differences and resemblances which are observed at present between the animals inhabiting different parts of the globe existed already in former geological periods. We must therefore become acquainted with the *general biological character* of the epoch as well as with the *local faunæ* of each period. The tertiary faunæ of New Holland and the Brazils for instance, resemble more closely the living faunæ of those parts of the world than they resemble one another. Our lists of fossils teem with chronological errors of the worst kind, arising partly from false identifications of strata, which in reality belong to different periods, but the fossils of which are thus represented as having inhabited our globe simultaneously, when in reality they may have been separated by long periods of time, and existed upon earth under very different physical conditions. This chronological confusion is further increased by the too extensive limits frequently assigned by geologists to the successive groups of rocks forming the crust of our globe. For instance, when the cretaceous or the oolitic formations are considered respectively as indivisible natural groups, and the fossils of all their subdivisions are enumerated in one single list as the inhabitants of a long period, an infinitude of anachronisms are presented to the mind, which no special mention of localities can rectify; and until the fossils of each of the natural subdivisions of these formations shall have been grouped together and compared carefully, as I have attempted to do it in my Monographs of the *Trigoniæ* and of the *Myæ* of Switzerland and the adjoining countries, or as Al. d'Orbigny has done it upon a much larger scale in his Paléontologie Française, no correct ideas can be formed respecting the succession of animals and plants characteristic of these long successive periods. I do not believe there is a single palæontologist, whose opinion is worth having, who can suppose, at this day, that any of the animals, the remains of which are buried in the lias, lived simulta-

neously with those of the inferior oolite, or these with those of the Oxford clay, or these with those of the upper division of the so-called oolitic formation. The same may be said of the different natural subdivisions of the cretaceous formation, and of the subdivisions introduced of late among the palæozoic rocks, by Sir Roderick Murchison, and Professor Sedgwick, and in America, by Professor J. Hall.

But even after this separation of the fossils, the synchronism of which may be fully established, our task is only fairly laid open, for then must begin the zoological identification of all the species, which has to be correct in every respect before general conclusions can be drawn from it.

In the first place the *specific identity* of organic remains is not so easily ascertained as many geologists would seem to suppose, if we judge from their statements; but unless the validity of a species is sanctioned by a practiced Zoölogist, it can not be taken as a basis for sound generalizations in reference to questions of a purely zoological character. The number of false identifications which have been accumulated in geological works is truly frightful. It would be however very unjust to accuse geologists in general of inaccuracy for this, the fault is mostly to be traced to other parties from which the names were obtained. It should only be understood that the materials thus accumulated are no longer fit to be used for the discussion of the questions which have been raised with the modern progress of geology, and that a thorough revision of *all* the identifications made in former years is imperatively demanded by the modern progress of palæontology. It would be however sometimes amusing, were it not actually distressing, to see the manner in which some geologists deal with fossils, considering them simply as the characteristics of certain rocks, and hardly yet dreaming that there may be such a thing as a special zoology of the different geological periods, and that during each, local faunæ may have existed with peculiar animals, &c. The ideas about characteristic fossils are still very crude, and nothing is more absurd than the complaints about unnecessary multiplication of genera and species; as if both genera and species had not a natural existence, independent of the estimates of naturalists. It would be just as reasonable for astronomers to complain of the great number of stars, as for geologists to object to the investigations of zoologists, on the ground that they lead to the "making" of "*too many species.*"

The difficulty with reference to the identification of species is three-fold: 1, different species may be considered as identical, 2, specimens of the same species in different states of preservation, or of different age, or sex, &c., may be considered as distinct species, or 3, the same species may have been described by different authors under different names, and their identity afterwards over-

looked by later writers. Who does not see what amount of error may accrue from the indiscriminate use of materials which are not first submitted to a very critical revision in these different respects, not to speak of the general difficulty of agreeing upon the limits of specific differences. With regard to this last point, however, I would say that whosoever would only use in discussing general questions materials revised candidly with the same principles, could not fail to obtain at least uniform results. And when the results of investigations made upon materials corrected in different ways by different authors are compared with one another, if these differences are kept in view, the disagreement in the results would not be found so great as it might otherwise seem. The astronomers and physicists have long learned to correct their observations before using them, and to take into consideration what they call the personal equation of different observers; are we never to learn from them a lesson in the estimation of our respective investigations, and shall our facts for ever be used without being first corrected for all the possible causes of error and disagreement? As long as there are differences of opinion respecting the natural limits of genera and species, is it not absolutely necessary to reduce or expand the scale applied to the investigations of different authors, when using them for the same purposes, exactly in the same manner as thermometric observations made with the scales of Reaumur or Celsius or Fahrenheit are reduced to the same standard, before being compared.

In the second place, *species must be referred to genera circumscribed within the same limits*, before they can fairly be compared or at least lead to trustworthy general results. As long as certain bivalve shells of the carboniferous and oolitic series were referred to the genus *Unio*, it could appear that the family of *Naiades* began its existence at a very early period; but since the oolitic species of this kind have been ascertained to differ essentially from our freshwater shells, and to constitute by themseles a natural genus more closely allied to Crassatella than to Unio, nobody thinks any longer of looking for Unios in *marine deposits*. As long as certain fossil fishes of the Zechstein and Lias were referred to the genera Esox and Cyprinus, the families of which these genera are the types could be supposed to have extended their range far beyond the tertiary formations, before which however no one of their representatives is to be found. Before the Spatangoids were divided into natural genera, the genus Spatangus was mentioned among the fossils of the oolitic as well as the cretaceous and tertiary formations; now it is restricted to the last among the fossils and found also among the living. I do not believe that a single genuine species of Gorgonia is found among the fossil Polypi, and yet that genus appears in the lists of fossils from the palæozoic period to the present time.

Since it is not my intention to enter here upon a special criticism of the innumerable errors of this kind, still to be found in even modern lists of fossils, I shall not multiply my examples. These may be sufficient to show how important a correct *generic* identification of the fossils may be in the estimation of the order of succession of organized beings; and I cannot but lament the utter want of consideration evinced even by many distinguished palæontologists in this respect, who seem to think that the knowledge of species is sufficient in itself to a proper appreciation of the order of creation, and that genera are arbitrary divisions established by naturalists merely for the sake of facilitating the study of species, as if the more general relations of living beings to one another were not as definitely regulated in all their degrees by the same thinking mind, as the ultimate relations of individuals to one another.

In the third place *the natural affinities of genera should be ascertained.* Unless the genera are referred to the families to which they truly belong, unless the rank of these families in their respective classes is positively determined, unless the peculiarities of structure which characterizes them is taken as the foundation of such an arrangement and further corroborated by the mode of development of their respective types, it would be a hopeless task to attempt to determine the order of succession of the fossils in different geological formations. Before the Crinoids which Lamark placed along the Polyps had been referred to the class of Echinoderms, nobody could have understood the beautiful gradation so fully ascertained now, which may be traced through all geological formations among these animals. Before it was ascertained that the little animal described by Thompson under the name of Pentacrinus europeus, as a living Crinoid, for which DeBlainville established the genus Phytocrinus, is in reality the young of a Comatula, nobody could have suspected the wonderful relations which exist between the changes animals now living undergo during their growth, and the order of succession of entire classes of animals during successive geological ages. As long as the natural position of Trilobites remained doubtful in the animal kingdom, the characters of the prototypes of the class of Crustacea could not be appreciated. Who does not see how impossible it was for those who classified the Trilobites with the Chitons to arrive at any sound results respecting the gradation and order of succession of these animals? Whilst now they are beautifully linked to the Macrura of the Trias, by the gigantic Entomostraca of the Devonian and Carboniferous periods. Again, the knowledge of the embryology of Crustacea gives us a key to a correct appreciation of the early appearance of the Macrura and the late introduction of the Brachyura. The removal of the Bryozoa from among Polypi to the class of Mollusks, will entirely

change the aspect and relations of the faunæ of the paleozoic rocks. How different, again, would the order of succession of Mollusks appear, were we to adhere to Cuvier's view of separating the Brachiopods, as a class, from the other Acephala, to which they are now more correctly referred The vexed question of the period of appearance of Dicotyledonous plants in the geological series would have been settled long ago, had it been placed upon its real foundation. It is not in reality to be argued upon palæontological evidence chiefly, for it resolves itself in the main into a botanical question, and the definite answer must depend upon the position finally assigned by botanists to the families of Coniferæ and Cycadeæ. If these natural orders of plants are really allied to the Dicotyledonæ, then this type begins with the palæozoic rocks in the Devonian system, and there is no gradation in the order of succession of plants during geological times. But if the view of Brongniart is more correct, if the Coniferæ and Cycadeæ have to be separated from the Dicotyledonæ as Gymnospermæ, and if moreover these latter should prove, as I believe they are, inferior even to the Monocotyledoneæ, then we may at once recognize in the vegetable kingdom a similar gradation of types as among animals. These examples may suffice to show what is required for a proper investigation of the order of succession of organized beings in the course of time, and how little confidence the investigations in this field deserve, which have not been made with due reference to all the points mentioned above. It is indeed only in the classes, the structure and embryology of which is equally well understood, we are able to discover the laws regulating the succession of animals and plants in geological formations, and our knowledge is at present still too imperfect to carry the investigation into all families of the animal kingdom. And yet enough is known to leave no doubt as to the final result; we may confidently await the time when the glory of the wonderful order of creation shall be fully revealed to us, and this may stimulate us to renewed efforts, since the success depends entirely upon our own exertions.

The geographical distribution of animals began only to be studied long after systematic zoology had made considerable progress, but even to this day the limits of the faunæ are nowhere circumscribed with any kind of precision, the principles upon which they might be determined are in many respects questionable, and a large number of animals are daily described without any reference to their natural distribution upon the earth; though much has already been done since Buffon to place this branch of our knowledge upon a better foundation, and especially to ascertain the laws regulating the geographical distribution of certain classes and families considered isolately. The point which requires now particular attention, is the combination of these differ-

ent types within definite regions, and their common circumscription within natural zoological provinces. This study would be particularly important with reference to the comparison of the local faunæ of former geological periods with those of the present creation. But since the latter even are comparatively little known, we must be satisfied to wait for the time when thorough comparisons shall be possible between the local faunæ of each and all geological periods *inter se*, and with those of other periods.

In closing this digression, I may sum up my criticism upon palæontological investigations, by saying that any generalization respecting the succession of organized beings which is not based upon materials in which the synchronism and succession of species and their geographical distribution is not duly considered, and in which the identification of species is not made with reference to sound zoological principles, with due regard to the equal limitation of genera, and also with reference to our improved classifications in zoology, is not fit to be trusted. All species taken into consideration should undergo a revision with reference to their chronology, their topography and their zoology, and in the last point of view the range and natural limitation as well as identity of the species, their generic affinities and their zoological classification should be equally tested.

Returning now to the main subject of this paper, I have further to say that the very fact that certain stratified rocks, even among the oldest formations, are almost entirely made up of fragments of organized beings, should long ago have satisfied the most skeptical that both *animal and vegetable life was as active and profusely scattered upon the whole globe, at all times and during all geological periods, as it is now.* No coral reef in the Pacific contains a larger amount of organic debris than some of the limestone deposits of the tertiary, of the cretaceous, or of the oolitic, nay even of the palæozoic periods, and the whole vegetable carpet covering the present surface of the globe, even if we were to consider only the most luxurious vegetation of the tropics, and leave entirely out of consideration the whole expanse of the ocean, as well as those tracks of land where under less favorable circumstances the growth of plants is more reduced, would not form one single seam of workable coal to be compared to the many thick beds contained in the rocks of the Carboniferous period alone.

ART. XXXIII.—*New Localities of Meteoric Iron;* by CHARLES UPHAM SHEPARD, M.D.

1. *Tazewell, Claiborne county, Tennessee.*

FOR the specimens of the highly interesting mass here described I am indebted to Prof. J. B. Mitchell, of the East Tennessee University, at Knoxville.* Though but a fragment of three $\frac{1}{16}$ths pounds, (having been detached from a mass originally weighing sixty pounds,) it nevertheless has much the appearance of an independent meteorite. Its shape is that of an elongated three-sided pyramid, whose axis is slightly oblique, and whose upper edges are obscurely truncated, so as to resemble an imperfectly formed six-sided pyramid of corundum. The height of the mass is four inches. The base is triangular and nearly smooth, presenting however a cleavage surface, partially coated by brown oxyd of iron. By this face, it was originally connected with the larger mass, of which it doubtless formed one of the sharpest extremities. It is certainly very remarkable that the cleavage should have been effected without leaving any hackly projections, the more so as the mass itself by no means yields to a similar cleavage in any direction whatever. Possibly the cleavage was occasioned by the interposition of a seam of pyrites, in the direction in which it took place. At any rate, its occurrence shows that these lumps, though generally composed of very tough and strongly coherent materials, are nevertheless susceptible of cleavage in certain directions, and that they may occasionally explode or subdivide into numerous fragments, without the necessity of any very considerable force.

The upper planes of the pyramid are indented, and somewhat undulating, as is usual in meteoric irons; but there is no thick

* The following is an abstract from Prof. Mitchell's letter in reference to its discovery: "This meteorite was found April, 1853, about ten miles west of Tazewell, Claiborne county, Tennessee. It was discovered by a son of William Rogers, while ploughing in clayey ground on the side of a hill, where the soil had been much washed away by rains. His attention was arrested by the resistance and noise produced, when the mass was struck by the plough. The lump weighed about sixty pounds. It was very irregular in its shape. The period of its fall is of course unknown. On account of its weight and lustre, it was regarded as silver; and it was with no small difficulty that the finder was induced to part with it, even by my paying him what appeared to be an equivalent, and then agreeing to give him its weight in silver, provided it should prove to be that metal, when properly examined. For my first information of the iron, I am indebted to J. C. Ramsay, Esq., a gentleman who, not limiting his researches to the mere details of his profession, loses no opportunity of contributing to several branches of natural history. I retain a fragment of about six ounces which he first gave me for examination. The remainder of a mass was broken when I saw it into two pieces, one of which, weighing perhaps three and a half pounds, I send to you. The largest portion, I transferred as I informed you, to an acquaintance for examining and reporting upon the same. These three embrace all the pieces into which this meteorite has been divided."

incrustation of peroxyd: on the contrary, it merely possesses for a coating a thin, brownish-black pellicle, which is much covered also by firmly adhering clay.

The iron is highly crystalline in its texture; a fact which may be seen in a few spots upon the surface, even through the coating itself. It is exceedingly tough, breaking with the greatest difficulty, and having a hackly surface, in which no crystallization is apparent. The fresh surface is much whiter than pure iron; and it retains its color and lustre apparently without change from ordinary exposure to the air. Its specific gravity = 7·30.

A part of the broad cleavage surface (or base of the pyramid) above described, was polished, and acted upon by dilute hydrochloric acid. The corrosion was very partial; but it revealed a perfectly crystalline structure in the iron. The subsequent application of nitric acid rendered it still more striking. The Widmannstättian figures are somewhat peculiar. While there is a general ground subdivided by innumerable thin and perfectly straight lines, into small equilateral triangles, and oblique-angled parallelograms of similar areas in size, presenting a picture on the whole closely resembling the Guildford (North Carolina) iron, there are also irregularly disposed veins, or interrupted seams, of a shining, white metal, $\frac{1}{20}$th of an inch in thickness, and each from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch long. These occur on the whole pretty near together, and impart a singular aspect to the surface, inasmuch as the veins do not coincide in direction with the fine lines above mentioned; nor do they follow any parallelism with one another.

Neither of the acids employed attack this substance in the slightest degree, any more than they do the thin lines producing the small and regular areas. But closely associated with the larger veins are noticeable small particles of magnetic pyrites, which as usual are decomposed by the acid.

Having separated a few grains of this metal or ore forming the seams, and heated it with acids, I convinced myself that it is identical with the substance which I discovered as entering into the composition of the Seneca Falls (New York) meteoric iron, and which I denominated Partschite.

A fragment of the iron was treated with hydrochloric acid. The solution went on very slowly, and unattended by the extrication of any sulphuretted hydrogen. The solution proceeded so slowly that it required nearly three days to dissolve 26·5 grs. of the iron, although the process of digestion was several times hastened by the application of a gentle heat. The acid left behind 1·16 grs. of undissolved matter, in the form of innumerable brilliant crystalline scales of an iron-gray color, and a high metallic lustre. When washed and dried, they were found to be flexible, highly magnetic, and insoluble in hydrochloric acid;

but were readily attacked by hot nitric acid, though still leaving undissolved a few particles of another metallic species, supposed to be the Partschite, and which were finally taken up by digestion in warm aqua regia. The thin, crystalline scales, undoubtedly consist of the schreibersite (of Patera).

From the hydrochloric solution a precipitate was obtained (by means of a stream of sulphuretted hydrogen), which, after washing and reduction before the blowpipe, yielded metallic copper. A solution of the perchlorid was precipitated by ammonia, and the peroxyd of iron thus obtained was ignited with nitrate of potassa; when its solution gave decisive evidence of the presence of chromic acid.

The proportion of nickel obtained from the iron (without including the schreibersite and partschite) was 12·10 to 13·05 p. c. —thus placing the present meteorite, as regards the high proportion of nickel, in the rank of the following rather small number of meteoric irons, viz., that of Caille, which has 17·37, of Claiborne, Alabama, which has 12·66, of Greene county, Tennessee 14·7, of Krasnojarsk 10·7, of Bitburg 11·9, and of Cape Colony, Africa 12·27.

I have abstained from a complete analysis of the present iron, as Prof. J. Lawrence Smith is, as I understand, about to publish full results of this nature.

### 2. *Haywood county, North Carolina.*

This specimen, which weighed scarcely $\frac{1}{8}$th of an ounce, was sent to me by Hon. T. L. Clingman, accompanied by the following remark: "It was given me by a person in Haywood county, whose father had obtained it in that region, but without his being able to designate the locality. It is evidently meteoric iron, but is perhaps from some mass already known."

The fragment is highly crystalline, and somewhat tetrahedral in form. One side was polished and etched. It displayed a marked character; and one which has no analogue among our meteoric irons. It is irregularly veined by a black ore, which was not acted upon by the acids; and which, when separated and submitted to examination, presented all the properties of magnetite. The general ground of the etched pattern is almost identical in character with that of the Hauptmannsdorf (Braunau) iron, it being characterized by this, that instead of brilliant projecting lines, it has fine, depressed lines, or grooves, which however are bright and glimmering in a strong light, and meet each other, mostly *at right angles*, parting off the ground into squares and rectangles, which are also to some extent, diagonally streaked, though always in a much fainter manner.

Specific gravity = 7·419. It dissolves in hydrochloric acid without the odor of sulphuretted hydrogen. The solution, when

perchloridized, gave with ammonia a precipitate which afforded distinct evidence of the presence of phosphorus and chromium: and the ammoniacal liquor was rich in nickel.

The small quantity of the iron at my disposal prevented a fuller examination of its properties. It is much to be desired that the present notice may lead to the discovery of the original mass.

### 3. *Union county, Georgia.*

For my specimen of this iron also, and for my chief information respecting its discovery, I am indebted to Hon. T. L. Clingman, as will appear from the following extract from his letter, dated Washington, Nov. 16, 1853. "Not long before I left home, I was at the copper mines of Dacktown, Polk county, Tennessee; and while there, in looking over some specimens of Mr. S. Congden, I found this ore, and told him that I was satisfied it was meteoric iron. He had taken it to be merely a rich iron-ore, and informed me that some weeks previously, it had been brought to him by a person, who had picked up a large lump of it in his field, and who had broken off this piece with a view to having it tested. The discovery was made in the edge of Georgia, but in what county Mr. Congden could not learn."

I have more recently ascertained from Mr. B. R. Dickey of Habersham county, Georgia, that the mass was found by a Mr. Freeman, in Union county of that state; and that its weight when picked up was about fifteen pounds.

The specimen sent to me by Mr. Clingman weighs one pound and one and three quarter ounces; and appears to have formed a portion of a somewhat tabular mass about two inches in thickness. It is coated on three sides, with a thin, scaly covering of brownish black hydrated peroxyd of iron. The other two sides present the appearance of a fresh fracture, but are nevertheless destitute of metallic lustre,—the surfaces being very irregular and dependant in form, upon the peculiar, concretionary character of the mass, which is strongly analogous to that of a very coarse-grained colophonite garnet, or the coccolite variety of pyroxene. It is, however, more or less traversed by cylindrical, or almond-shaped masses of meteoric pyrites, some of which are above an inch in length and one-third of an inch in diameter.

When polished, it approaches more nearly to a silver-white color, than any other meteoric iron. When acted upon by acids, it does not give the Widmannstättian figures; but only develops a series of web-like meshes, or, at most, a mottled, map-like delineation.

Its specific gravity $= 7{\cdot}07$. A fragment, as nearly as possible free from pyrites, was found to contain 3·32 p. c. of nickel. It is rich in chromium, and contains traces of phosphorus, cobalt, magnesium and calcium.

4. *Meteoric Iron? from Long creek, Jefferson Co., Tennessee.*

This iron was forwarded to me by Judge J. Peck of Oakland, East Tennessee, (to whom we owe the discovery of the Cosby Creek iron, and that from Green county in the same state) with the following remarks respecting its discovery. "It was found on Long creek, Jefferson county, a few miles north of the mouth of Chucky creek. Before I got possession of it (which was accidental) it had fallen into the hands of a young vulcan, who would try its metal, as you see. I regret its being disfigured: but I have saved all I could. Near the same place, another piece was picked up, of which, however, I possess a mere fragment; but it appears to be quite identical with the specimen I send. The pieces are described to me as being of about the same size and not unlike in figure. The family who made the discovery, affirmed to me, when I visited the locality, that there was a vein of it, and that wagon loads could be picked up. But the creek where the vein was said to be, was too high. I made a liberal offer for all they could find and bring to me, in an unaltered condition; but I hear of no more. Feb. 7, 1853."

The mass sent, weighed about two and a half pounds, and had an oval flattened form, much like a thick, freshwater bivalve shell. It is somewhat flattened on one side, and presents a broad fractured surface on one side, as if it had been broken from a larger mass. The natural outside is somewhat undulating and pitted, and bears some marks of having been heated and hammered. But it exhibits a very marked peculiarity in being coated almost uniformly by a layer one-twentieth of an inch thick, of specular iron, which has a botryoidal surface, and a concentric, lamellar structure. This coating moreover, often penetrates by little irregular veins, for half an inch or more, into the substance of the mass.

It breaks with difficulty; at first, slightly flattening under the hammer. The fresh fracture shows no bright metallic points. It presents a granular surface, much resembling certain rather fine grained blackish chlorites, or some varieties of graphite. Here and there throughout its mass, are noticeable rounded globules of metal from $\frac{1}{50}$th to $\frac{1}{6}$th of an inch in diameter. These are quite smooth externally; and when detached from their bedding, leave behind a smooth corresponding cavity. The material of the globules does not differ sensibly from the rest of the mass, except in being rather finer grained.

Specific gravity = 7·43. When polished, it presents a dark iron-grey color, and an imperfectly metallic lustre. Acids do not develop any crystalline texture with this iron.

It dissolves easily in hydrochloric acid, with no perceptible odor of sulphuretted hydrogen, but nevertheless slightly discoloring a

paper wet with a solution of acetate of lead, when held over the solution. A mineral resembling graphite rapidly separates as the solution goes on, and floats through the liquid in small shining black scales of perfectly uniform size. When no heat is employed in the process, their form is very definite, and on being washed and dried, they gave in one instance a specific gravity of 3·30. Deflagrated with nitrate of potassa they gave decided traces of titanic and silicic acids. Where heat and aqua regia were employed in acting upon the iron, the graphite-like substance was more acted upon, and had a specific gravity of but 2·20. This when deflagrated with nitre also afforded traces of silicic and titanic acids. The proportion of the heavy graphite in the iron, obtained in the first process was 4·5 p. c.; but by the latter process it was 3·3 p. c.

A portion of the iron was heated with hydrochloric acid, and the solution saturated with sulphuretted hydrogen. A yellowish brown precipitate was formed. It was separated and treated with concentrated nitric acid. A heavy, white powder was formed, which was not reduced in quantity by further digestion in the acid. It was separated, washed, dried and ignited, and amounted to 0·4 gr. or 5·50 grs. of the iron.

A portion of the nitric acid solution, on being treated with sulphuretted hydrogen, gave a brown precipitate of sulphuret of molybdenum. Another portion gave with tannic acid, a yellow brown precipitate; and, another still, gave with proto-nitrate of mercury, a yellow precipitate of molybdate of mercury, which was dissolved by nitric acid. The iron affords decisive indications of the presence of phosphorus and of chromium, when subjected to the usual tests for these elements; but does not contain nickel, cobalt, magnesium or calcium. Besides the specular iron of the crust, therefore, it contains,

| | |
|---|---|
| Iron, | 95·575 |
| Carbon, | 3·30 |
| Chromium, Tin, Molybdenum, | 1·125 |
| Silicon, Titanium, Phosphorus, Sulphur, | *traces* |
| | 100·00 |

ART. XXXIV.—*Description of Meteoric Iron from Putnam County, Georgia;* by J. E. WILLET, Professor in Mercer University, Geo.

THIS interesting meteoric iron, the first that has been found in Georgia, was presented to Mercer University by John A. Cogburn, Esq., in the fall of 1852.

The circumstances of its discovery, as detailed by Mr. Cogburn, are briefly these. The iron was first observed by his overseer, in 1839, in a field which had been cultivated for several years; but was supposed to be the common black rock of that region. Mr. Cogburn first noticed it March, 1840, and, attempting to raise it from the ground, found it so heavy that he carried it to his blacksmith shop to have it broken. Its weight, at that time, was 72 pounds, and the mass was coated deeply with a brown, scaly crust. He attempted to break it upon an anvil, but could remove only the outside crust, including a large blister, the place of which is now indicated by a deep fissure. Finding it so untractable, he threw it out into his yard, where it lay neglected; until a knowledge of the fact led me to request him to send it to the University for examination. He states further, that he supposes it to have been originally buried, and brought to the surface of the earth by cultivation and the action of rains; that there is no tradition of its fall; and that no similar pieces have been found in the neighborhood.

Its weight, when it was brought to the University, was about sixty pounds. In shape, it represents a rude triangular pyramid, with its base and edges rounded, and its faces exposing many knobs and depressions.

Most of the crust has been removed by the rough handling which it has received. The outer layers of what remains separate in thin scales of no regular shape; the inner portions break into rhombohedral masses, which, under the influence of a magnet, become permanently magnetic; showing that the iron has here been converted into magnetic oxyd. The mass of iron exhibits no magnetism.

In removing a slab, the iron was found to be remarkably tough and compact. The torn edges oxydized rapidly and developed the crystalline structure, before the application of acid; the oxydation proceeding inwardly from the edges and following the lines of cleavage first, and afterwards spreading over the inclosed areas. The sawn surfaces, after a few days exposure, were found bedewed with drops of a liquid, supposed to be chlorid of iron. After longer exposure, the exudation has ceased; a point of striking similarity with the Texas iron. The polished surface is uniform, without markings, and with few flaws.

Hydrochloric acid, applied to the heated slab, attacks it, with a rapid evolution of hydrogen bubbles, but develops only a few

of the larger bars; and the crystalline structure of the mass might be overlooked with the action of this acid alone. Nitric acid, however, brings out the Widmanstättian figures most beautifully. The etched surface is a perfect miniature copy of the Texan iron; the largest bars of the Putnam county iron corresponding with those of medium size in the Texas iron, and thence diminishing to bundles of striæ hardly visible to the naked eye. The triangles and parallelograms are proportionally small. Query: Are the crystalline figures of meteoric irons, in any degree, proportional to the meteoric masses? If so, may we not infer from the size of them, whether the iron be an entire mass, or a fragment of a large one?

Neutral sulphate of copper produces no precipitate of metal on the iron; the slightest addition of acid causes the deposit of copper. Moreover, I find, that if the film of copper be wiped off as soon as formed, the sulphuric acid has etched out the figures superficially but very imperfectly. Liquid sulphuric acid, when cold, has no effect upon the surface.

In addition to the above description, I subjoin an interesting note from Prof. Shepard, containing an analysis of the iron, which he has very kindly furnished at my request.

"Charleston, S. C., February 10, 1854.

*My Dear Sir*—In comparing the Putnam county meteoric iron with specimens from other localities, I notice a striking similarity in its structure to that of the Texan mass. Like it, your iron is compact, nearly free from pyrites and but slightly disposed to rust on exposure to the air. But the resemblance between the two is seen to the greatest advantage, when etched samples are compared with one another. The Putnam county iron exhibits figures of the same shape and size as the Texas, viz., triangles and oblique-angled parallelograms, bounded by slightly raised edges, which are often wavy, and sometimes not continuously of the same thickness, but, here and there, bulging out into beads or knobs. The pyrites in my specimens is scarcely to be recognized, except in one or two very limited patches, which are irregular and vein-like.

The iron appears to have suffered a very remarkable disintegration to the depth of half an inch or more below the thick, scaly crust with which the mass was coated; in consequence of which it cleaves very regularly, like the Cocke county, Tennessee iron, into tetrahedral and rhomboidal fragments.

The specific gravity of the fresh internal portions of the mass is 7·69. A single analysis gave me the following result:

| | |
|---|---|
| Iron, - - - - - - - - - - | 89·52 |
| Nickel, with traces of cobalt, - - - - - | 8·82 |
| Tin, phosphorus, sulphur, magnesium and calcium, | 1·66 |
| | 100·00 |

Very truly, yours,

CHARLES UPHAM SHEPARD."

ART. XXXV.—*On Conistonite, a new Mineral Species;* communicated by R. P. GREG, F.G.S., of Norcliffe Hall, near Manchester.

Mr. B. WRIGHT, a mineral dealer in Liverpool, forwarded me some months since, a specimen, found at the copper mine near Coniston in Cumberland, by Mr. Marrat, a teacher of Natural History in Liverpool. The specimen was of a purplish-red color, somewhat resembling earthy cobalt-bloom, and implanted on it were a few small crystals resembling calcite. Thinking however the form of these rather peculiar, on trial I found the usual cleavages of that mineral wanting; and its peculiar behavior before the blowpipe soon convinced me that it must be a new species. I give the following general description:

Largest crystals not more than one-eighth of an inch across, and in general form not unlike the double four-sided crystals of Edingtonite. Primitive form probably a right rhombic prism. No cleavage observable. Fracture small conchoidal, uneven. Lustre vitreous. Transparent to translucent; colorless. Slightly sectile. H. = 2·2. Sp. gr. = 2·05.

M : M′ = 97·05
M : *e* = 122·50
*e* : *e* = 94·00

The faces M brighter and more distinct than *e*.

Does not effervesce in acids. Before the blowpipe becomes white and opaque, expanding into seven or eight times its original bulk. After exposure to heat almost instantaneously dissipated in acids, with strong effervescence.

As yet only two specimens of this interesting mineral have been found. I have called it Conistonite from its having been first discovered at Coniston.

It is very probable that the matrix in which the crystals of Conistonite are imbedded, will itself prove to be a new mineral species. Mr. Heddle supposes it to be oxalate of cobalt.

I transmitted to my friend, Mr. M. Forster Heddle, of Edinburgh, a few crystals of the Conistonite for analysis, which he kindly undertook to make; and he has sent me the following particulars respecting it.

*a.* "Sp. gr., determined on 3·666 grs., found to be 2·052.

*b.* Powdered mineral absorbs ·23 per cent. of moisture.

*c.* Soluble without effervescence in hydrochloric and nitric acids, and on addition of ammonia a precipitate is thrown down.

*d.* When heated gives off water and carbonic oxyd, and is converted into carbonates; and *then* effervesces in acids.

*e.* Qualitative analysis made on a very minute quantity, gave indications of lime, magnesia, water and oxalic acid.

*f.* Quantitative analysis made on 3·83 grs.:

| | | | | | |
|---|---|---|---|---|---|
| Carbonate of lime, | 37·597 | | Lime, - | 21·055 | 21·877 |
| Water, and Carbonic oxyd, | 60·052 | which corresponds to | Soda and Magnesia, | ·822 | |
| Carbonates of soda and magnesia in nearly equal quantities, | 1·400 | | Oxalic acid, - | 28·017 | |
| | | | Water, - - | 49·155 | |
| | 99·049 | | | 99·049 | |

*g.* Uniting the soda and magnesia with the lime to calculate the formula, we get,

| | | Atom. |
|---|---|---|
| Lime, - - - | 21·877 = | 625 = 1 |
| Oxalic acid, - - | 28·017 = | 622 = 1 |
| Water, - - - | 49·155 = | 4369 = 7 |

which gives the formula $CaO, C_2O_3, 7HO$; being an oxalate of lime with 7 atoms of water instead of *one;* or more probably oxalate of lime with 6 atoms of water of crystallization, thus $CaO, C_2O_3, HO+6HO$; or $\dot{C}a\bar{C}+7\dot{H}$.

*h.* The great interest of the mineral lies in the fact of the water of crystallization, rendering oxalate of lime dimorphous, the Whewellite of Brooke being oblique.

It is curious that the specific gravity of Conistonite should be greater than Whewellite; we should have expected that the additional HO would have lowered the gravity."

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ART. XXXVI. — *Introductory Essay, in Dr. Hooker's Flora of New Zealand.*

(Concluded from p. 252.)

HYBRIDIZATION has been supposed by many to be an important element in confusing and masking species. Botanists of one class are apt to refer to its agency the unwelcome appearance of a specimen which combines two nominal species, founded on inconstant characters. Another class of naturalists appeal to the repeated occurrence of fertile hybrids, to negative the inference, otherwise unavoidable, that the production between two individuals of inherently fertile offspring, is a good reason for pronouncing them identical in species. Any continued effects from hybridization in uncontrolled nature seem to be thoroughly guarded against in two ways; first by the constitutional debility, if not by the invariable sterility of the hybrid offspring, rendering it of transient duration; and secondly, by the fact that, when prolific at all, they usually become so through fertilization by one or the

other of the parents, when the offspring reverts to that specific type. Dr. Hooker justly remarks, that,

"As a general rule, the genera most easily hybridized in gardens are not those in which the species present the greatest difficulties. With regard to the facility with which hybrids are produced, the prevalent ideas on the subject are extremely erroneous. Gærtner, the most recent and careful experimenter, who appears to have pursued his inquiries in a truly philosophical spirit, says that 10,000 experiments upon 700 species produced only 250 true hybrids. It would have been most interesting had he added how many of them produced seeds, and how many of the latter were fertile, and for how many generations they were propagated. The most satisfactory proof we can adduce, of hybridization being powerless as an agent in producing species (however much it may combine them), are the facts that no hybrid has ever afforded a character foreign to that of its parents, and that hybrids are generally constitutionally weak, and almost invariably barren. Unisexual trees must offer many facilities for the natural production of hybrids, which nevertheless have never been proved to occur; nor are such trees more variable than hermaphrodite ones."

May we not even say that they are less variable *because* unisexual, since their progeny is most likely to be originated from the conjunction of different trees, and individual peculiarities must needs be thereby blended and obliterated. When we consider how often it must happen that the ovules of one tree in an oak or pine forest are fertilized by the pollen of another; when we take into view the great number of unisexual plants, and consider also how many apparently hermaphrodite blossoms (far more than is generally supposed) are really submonœcious or polygamous, we may find reason to suspect in this a more general provision of nature for limiting congenital or induced individual forms than would at first appear. That some such controlling or amalgamating agencies operate in nature may be inferred from a comparison of the general homogeneity of an indigenous species, over even a large area, with the ready development of marked varieties or races in the case of every cultivated plant which is multiplied from seed, and their perpetuation from generation to generation, which is almost always ensured merely by cultivating each stock by itself. For the general law of nature is, not merely that *species* are true to their characters, these characters being rightly apprehended, but that the *individual* progeny inherits and transmits the special peculiarities of the parent or parents. Hence, that an isolated race retains certain characteristics so long as kept separate is no proof that it constitutes a species. Many a variety of recent and known origin does this. But, on the other hand, whatever individuals, however distinguished by minor differences when separate, are found to blend into a fertile race when associated, must on sound principles be regarded as belonging to one species. Perhaps if zoologists would contemplate the wide varia-

tions presented by many plants of indubitably one and the same species, and the still wider diversities of long cultivated races from an original stock, they would find more than one instructive parallel to the case of the longest-domesticated of all species, *man*.

Let it also be especially noted, that varieties are not always, not even generally, the result of external agencies, at least of such as we are able to detect. Certain varieties of plants are so originated: these are generally as transient as the cause that produces them, and under altered circumstances often disappear even during the life of the individual: the plant may outgrow them. But others arise, we know not how. It is past the wit of man to trace a connection between the diverse forms of the foliage, &c. of a polymorphous plant such as a *Coprosma*, *Metrosideros* or *Alseuosmia* in New Zealand, and any difference of circumstances attributable to station; still more so when these diversities occur side by side. Yet such are the varieties which ordinarily exhibit the greatest persistency, i. e. when kept from intermingling by mutual fecundation. What external circumstances can in the least account for the origin of the race of Dorking fowls, or Manx cats, or indeed of almost any of our domesticated races which were not produced by cross-breeding, that is by mingling the characters of two such races already in existence. Yet how certainly may we continue any of these races, under favorable circumstances, merely by maintaining the needful isolation; and how soon would they mingle and disappear if they were left to themselves. Is not this equally true of the human races?

From such facts, from the study of species remarkable for their polymorphous vegetation, as well as from the wide differences between certain domesticated races and their original stock,—differences not to have been expected on any known principles, and often far greater than those which are reckoned satisfactorily to mark the limits between other species,—the systematic botanist, at least, learns the lesson, that the amount of variation of which any one specific type is susceptible is only to be ascertained from observation and experience.

Upon the third proposition, that "Species are more widely diffused than is usually supposed," our author's statements are brief, but very decided and remarkable. He has already declared in the previous section "that it is by far the smaller half of the vegetable kingdom that is confined to narrow geographical or climatic areas, and that very few plants indeed are absolutely local; whilst the operations of the gardener and agriculturist prove that a vast proportion of the plants of the two temperate zones are capable of growing in any moderate climate." "I do not think," he continues, "that those who argue for narrow limits to the distribution and variation of species, can have considered a garden in a philosophical spirit, or have weighed such facts as

that there have been cultivated, within the last seventy years in the open air of England (at Kew), upwards of 20,000 species of plants from all quarters of the globe, and this within a space that, had it been left to nature, would not have contained 200 indigenous species. The fact that an overwhelming proportion of these have come up true to their parent, and have continued so under every possible disadvantage of transportation and transplantation, of altered seasons, and amount and distribution of temperature and humidity, of unsuitable soil and exposure, and of the multitude of errors which unavoidable ignorance of their natural locality and habit engenders: such appears to me the most forcible argument in favor of the power of plants to retain their original characters under altered circumstances."

The validity of the last inference is unquestionable and valuable. As long as the plants survived, they doubtless retained their characters. But the value of the general statement, as respects its bearing upon the natural diffusion of species, depends much on the answer to the question, how long did the majority of the species survive. We should like to know how large a proportion continued beyond the first two or three years, even with all the advantages of being looked after by the gardener.

But, returning to the general question, we remark, that there is little or no reason to expect a similarity between plants and animals in this respect. From the nature of the case we should suppose that the latter, at least that land-animals, would be much more local than plants.

We give to our readers the whole of the third section; as some of the statements, especially those that relate to the probable number of vegetable species known, are likely to excite some surprise.

"This is a point upon which my own views differ materially from those of many of my fellow botanists, and which, if borne out by facts, leads to a widely different estimate of the number and variety of the members of the vegetable kingdom than that which is at present entertained. As with the affinities and variation of species, so is it with their distribution: an extensive knowledge of the subject is only to be obtained by actual observation over large areas, and many of them, or by the study and comparison of the contents of many museums. It has been my singular good fortune to have visited many regions of the globe, and to have entered into some details upon the dispersion of living species, which has always been a favorite pursuit of mine. I have further had the advantage of collating my results with the largest and best-named botanical collections in the world, and have received a greater amount of assistance from my fellow naturalists than has fallen to the lot of most: facts which in ordinary cases are the result of long study and much consultation have been placed at my disposal rather than worked out by myself. A very extended examination of these materials has only tended to confirm the view which originated in my

personal experience, viz., that the estimate of the number of species known to botanists is a greatly exaggerated one,* and the prevalent ideas regarding their distribution no less contracted.

Many more plants are common to most countries than is supposed; I have found 60 New Zealand Flowering plants and 9 Ferns to be European ones, besides inhabiting various intermediate countries; and amongst the lower Orders we find a greatly increased proportion of species common to all countries: thus of Mosses alone 50 are found in New Zealand and Europe;† of Hepaticæ 13; of Algæ 45 are also natives of European seas; of Fungi nearly 60; and of Lichens 100.

So long ago as 1814 Mr. Brown‡ drew attention to the importance of such considerations, and gave a list of 150 European plants common to Australia. The identity of many of these has repeatedly been called in question, but almost invariably erroneously; added to which more modern collectors have greatly increased the list.

The too prevalent idea that the plants of newly discovered, isolated, or little visited localities must necessarily be new, has been a fertile source of the undue multiplication of species. There are very many cases of naturalists having been so impressed with this idea, that they have not thought it worth while to consult either books or herbaria before describing the plants from such spots. The New Zealand Flora presents several instances of this; two conspicuous ones occur in the genus *Oxalis;* one, *O. corniculata*, is amongst the most widely diffused and variable plants in the world; of its varieties no less than seven or eight species have been made, most of them supposed to be peculiar to New Zealand; not only is *O. corniculata* hence excluded from the flora, but, in the descriptions of these its varieties, no allusion is made to that plant.§ In the case of the other species the error is more ex-

* According to the loose estimate of compilers, 100,000 is the commonly received number of known plants: from a multiplicity of data I can come to no other conclusion but that half that number is much nearer the truth. This may well be conceived, when it is notorious that nineteen species have been made of the common Potato, and many more of *Solanum nigrum* alone. *Pteris aquilina* has given rise to numerous book species; *Vernonia cinerea* of India to fifteen at least. Many of the commonest European plants have several names in Europe, others in India, and still others in America, besides a host of garden names for themselves, their hybrids and varieties, all of which are catalogued as species in the ordinary works of reference whence such estimates are compiled.

† In fact the distribution of some Cryptogams is so wide, that I have visited a spot in a high southern latitude, nearly all whose plants are not only identical with those of Great Britain, but inhabit many intermediate temperate and tropical countries. Cockburn Island, in lat. 64° 12′ S. and long. 64° 49′ W., nearly fulfills this condition; I thereon collected nineteen plants, of which three-fourths are natives of England.

‡ Appendix to Flinders's Voyage, vol. ii, p. 592.

§ I have stated very confidently in the body of this work that eight of Cunningham's and Richard's species of this genus are all referable to one. This view will probably not meet the approbation of the local botanists, who will point to the constancy with which some of the states retain their characters under varied conditions. I value such facts very highly, and attach great weight to them, and did these varieties occur only in New Zealand I should perhaps have withheld so strong an opinion on the subject; but such is not the case. *O. corniculata* varies as much in numerous other parts of the world; and admitting, as every one must, that varieties are known to retain their characters with more or less constancy for certain periods, some other evidence is necessary to shake the opinion of the botanist who grounds his views on an examination of the plant from all quarters of the globe.

cusable, and may be still open to question;* it is that of *O. Magellanica*, originally discovered in Fuegia, and imperfectly described by Forster, whose very indifferent specimens of it are in the British Museum. When re-found in New Zealand it was described as new, and called *O. cataractæ*, and when found a third time in Tasmania, was called by still a third name, *O. lactea*. In this case a more important fact was smothered than that of the distribution of *O. corniculata*, namely, that of a very peculiar plant of the south temperate zone being common to these three widely sundered localities.

Many similar instances might be added, for there are several New Zealand plants (as *Pteris aquilina*) that have a different name in almost every country in the world: and, partly from changes in nomenclature, partly from the reduction of species, I have found myself obliged to quote 1500 names for the 720 New Zealand Flowering plants described; and I believe I might have doubled the number had my limits not obliged me to reduce the synonymy as much as possible; in many cases too much, I fear, for the requirements of working botanists in Europe."

One thing is clear, and important to be enforced: namely, that if determinations of species are to be of any value, especially in their bearing on general questions, they must rest solely upon observed characters of admitted value, irrespective of all theory. We pronounce such and such individuals, from a certain habitat, to belong to a distinct species, only because we find them possessed of certain adequate distinctive marks. If we at length ascertain that particular species are peculiar to particular stations or parts of the world, we have a sound and valuable deduction. But to assume that certain plants, or certain animals, from widely sundered localities belong to different species, notwithstanding their resemblance, until the contrary is proved, and even to announce this as a principle for general adoption, as has been done, is surely a gross instance of reasoning in a vicious circle.

We cannot venture to condense, and therefore reproduce the whole of § 4, viz.:

"*The distribution of species has been effected by natural causes, but these are not necessarily the same as those to which they are now exposed.*

Of all the branches of Botany there is none whose elucidation demands so much preparatory study, or so extensive an acquaintance with plants and their affinities, as that of their geographical distribution. Nothing is easier than to explain away all obscure phenomena of dispersion by several speculations on the origin of species, so plausible that the superficial naturalist may accept any of them; and to test their soundness demands a comprehensive knowledge of facts, which more-

* As no identification is proved till all the organs of the plants to be compared have been studied, there is yet a possibility of these three species proving distinct, but I do not at all expect it; the only difference I can find is a greater obliquity and emargination of the petals of the New Zealand species, but that character varies so much both in this plant and in others of the genus that it loses all specific value.

over run great risk of distortion in the hands of those who do not know the value of the evidence they afford. I have endeavored to enumerate the principal facts that appear to militate against the probability of the same species having originated in more places (or centres) than one; but in so doing I have only partially met the strongest argument of all in favor of a plurality of centres, viz., the difficulty of otherwise accounting for the presence in two widely sundered localities of rare local species, whose seeds cannot have been transported from one to the other by natural causes now in operation. To take an instance: how does it happen that *Edwardsia grandiflora* inhabits both New Zealand and South America? or *Oxalis Magellanica* both these localities and Tasmania? The idea of transportation by aerial or oceanic currents cannot be entertained, as the seeds of neither could stand exposure to the salt water, and they are too heavy to be borne in the air. Were these the only plants common to these widely-sundered localities, the possibility of some exceptional mode of transport might be admitted by those disinclined to receive the doctrine of double centres; but the elucidation of the New Zealand Flora has brought up many similar instances equally difficult to account for, and has developed innumerable collateral phenomena of equal importance, though not of so evident appreciation. These, which all bear upon the same point, may be arranged as follows:—

1. Seventy-seven plants are common to the three great south temperate masses of land, Tasmania, New Zealand, and South America.

2. Comparatively few of these are universally distributed species, the greater part being peculiar to the south temperate zone.

3. There are upwards of 100 genera, subgenera, or other well-marked groups of plants entirely or nearly confined to New Zealand, Australia, and extra-tropical South America. These are represented by one or more species in two or more of these countries, and they thus effect a botanical relationship or affinity between them all, which every botanist appreciates.

4. These three peculiarities are shared by all the islands in the south temperate zone (including even Tristan d'Acunha, though placed so close to Africa), between which islands the transportation of seeds is even more unlikely than between the larger masses of land.

5. The plants of the Antarctic islands which are equally natives of New Zealand, Tasmania, and Australia, are almost invariably found only on the lofty mountains of these countries.

Now as not only individual species, but groups of these, whether orders, genera, or their subdivisions, are to a great degree distributed within certain limits or areas, it follows that the flora of every island or archipelago presents peculiarities of its own. Though an insular climate may favor the relative abundance of individuals, and even species of certain Natural Orders, there is nothing in the climate, or in any other attribute of insularity, which indicates the nature of the peculiarity of endemic species. The islands of each ocean contain certain botanically allied forms in common, which are more or less abundant in them, and rarely or never found on the neighboring continents; thus there are curious genera peculiar to the North Atlantic islands, others to the North Pacific islands, others to those of the South Pacific and others again to the Malayan Archipelago; just as there are still

others peculiar to the Antarctic islands, and many to New Zealand, Fuegia, and Tasmania.

Each group of islands hence forms a botanical region, more or less definable by its plants as well as by its oceanic boundaries; precisely as a continuous area like Australia or South Africa does. There is however this difference, that whereas the Natural Orders that give a botanical character to a continuous area of a continent or to a large island (as the *Proteaceæ* in South Africa or in New Holland, and *Coprosma* in New Zealand) are numerous in species and often uniformly spread,—in clusters of small islands, distant from continents, they are few in species, and the individuals are scattered, appearing like the vestiges of a flora which belonged to another epoch, and which is passing away. This is perhaps a fanciful idea, but one which I believe to contain the germ of truth; for no Botanist can reflect upon the destruction of peculiar species on small islands (such as is now going on in St. Helena amongst others), without feeling that, as each disappears, a gap remains, which may never be botanically refilled; that not only are those links breaking by which he connects the present flora with the past, but also those by which he binds the different members of the vegetable kingdom one to another. It is not true in every sense that all existing nature appears to the naturalist as an harmonious whole; each species combines by its own peculiarities two or more others more closely, and reveals their affinities more clearly, than any other does; just as the flora of an intermediate spot of land connects those of two adjacent areas better than any other locality does. It is often by one or a very few species that two large Natural Orders are seen to be related; just as by a few Chilian plants the whole flora of New Zealand is connected with that of South America. The destruction of a species must hence create an hiatus in our systems, and I believe that it is mainly through such losses that natural orders, genera, and species become isolated, that is, peculiar, in a naturalist's eyes.

To return to the distribution of existing species, I cannot think that those who, arguing for unlimited powers of migration in plants, think existing means ample for ubiquitous dispersion, sufficiently appreciate the difficulties in the way of the necessary transport. During my voyages amongst the Antarctic islands, I was led, by the constant recurrence of familiar plants in the most inaccessible spots, to reflect much on the subject of their possible transport; and the conviction was soon forced upon me, that, putting aside the almost insuperable obstacles to trans-oceanic migration between such islands as Fuegia and Kerguelen's Land, for instance (which have plants in common, not found elsewhere), there were such peculiarities in the plants so circumstanced, as rendered many of them the least likely of all to have availed themselves of what possible chances of transport there may have been. As species they were either not so abundant in individuals, or not prolific enough to have been the first to offer themselves for chance transport, or their seeds presented no facilities for migration,* or were singularly perishable from feeble vitality, soft or brittle integuments, the presence of oil that soon became rancid, or from having a fleshy albumen that

* Thus of the *Compositæ*, common to Lord Auckland's Group, Fuegia, and Kerguelen's Land, none have any pappus (or seed-down) at all! Of the many species *with* pappus, none are common to two of these islands!

quickly decayed.* Added to the fact that of all the plants in the respective floras of the Antarctic islands those common to any two of them were the most unlikely of all to emigrate, and that there were plenty of species possessing unusual facilities, which had not availed themselves of them, there was another important point, namely, the little chance there was of the seeds growing at all, after transport. Though thousands of seeds are annually shed in those bleak regions, few indeed vegetate, and of these fewer still arrive at maturity. There is no annual plant in Kerguelen's Land, and seedlings are extremely rare there; the seeds, if not eaten by birds, either rot on the ground or are washed away; and the conclusion is evident, that if such mortality attends them in their own island, the chances must be small indeed for a solitary individual, after being transported perhaps thousands of miles, to some spot where the available soil is pre-occupied.

Beyond the bare fact of the difficulty of accounting by any other means for the presence of the same species in two of the islands, there appeared nothing in the botany of the Antarctic regions to support or even to favor the assumption of the double creation, and I hence dismissed it as a mere speculation which, till it gained some support on philosophical principles, could only be regarded as shelving a difficulty; whilst the unstable doctrine, that would account for the creation of each species on each island by progressive development on the spot, was contradicted by every fact.

It was with these conclusions before me, that I was led to speculate on the possibility of the plants of the Southern Ocean being the remains of a flora that had once spread over a larger and more continuous tract of land than now exists in that ocean; and that the peculiar Antarctic genera and species may be the vestiges of a flora characterized by the predominance of plants which are now scattered throughout the southern islands. An allusion to these speculations was made in the 'Flora Antarctica' (pp. 210 and 268), where some circumstances connected with the distribution of the Antarctic islands were dwelt upon, and their resemblance to the summits of a submerged mountain chain was pointed out; but beyond the facts that the general features of the flora favored such a view, that the difficulties in the way of transport appeared to admit of no other solution, and that there are no limits assignable to the age of the species that would make their creation posterior to such a series of geological changes as should remove the intervening land, there was nothing in the shape of evidence by which my speculation could be supported. I am indebted to the invaluable labors of Lyell and Darwin,† for the facts that could alone have given

* Of the seeds sent to England from the Antarctic regions, or transported by myself between the several islands, almost all perished during transmission.

† See Darwin's 'Journal of a Naturalist,' and 'Essays on Volcanic Islands and Coral Islands.' The proofs of the coasts of Chili and Patagonia having been raised continuously, for several hundred miles, to elevations varying between 400 and 1300 feet, since the period of the creation of existing shells, will be found in the first-named of these admirable works, which should be in the hands of every New Zealand Naturalist, if only from its containing important observations on his own islands. The fact of this accomplished Naturalist and Geologist having preceded me in the investigation of the Natural History of the Southern Ocean, has materially influenced and greatly furthered my progress; and I feel it the more necessary to

countenance to such an hypothesis; the one showing that the necessary time and elevations and depressions of land need not be denied; and the other, that such risings and sinkings are in active progress over large portions of the continents and islands of the southern hemisphere. It is to the works of Lyell* that I must refer for all the necessary data as to the influence of climate in directing the migration of plants and animals, and for the evidence of the changes of climate being dependent on geological change. In the 'Principles of Geology' these laws are proved to be of universal application, and amply illustrated by their being applied to the elucidation of difficult problems in geographical distribution. It follows from what is there shown, that a change in the relative positions of sea and land has occurred to such an extent, since the creation of still existing species, that we have no right to assume that the plants and animals of two given areas, however isolated by ocean, may not have migrated over pre-existing land between them. This was illustrated by an examination of the natural history of Sicily (where land-shells, still existing in Italy, and which could not have crossed the Straits of Messina, are found imbedded on the flanks of Etna high above the sea-level), regarding which Sir Charles Lyell states that most of the plants and animals of that island are older than the mountains, plains, and rivers they now inhabit.†

It was reserved for Professor Edward Forbes, one of the most accomplished naturalists of his day, to extend and enlarge these views, and to illustrate by their means the natural history of an extensive area; which he did by applying a profound knowledge of geology and natural history to the materials he had collected during his arduous surveys of many of the shores of Europe and the Mediterranean. The result has been the enunciation of a theory, from which it follows that the greater part, if not all, of the animals and plants of the British Islands have immigrated at different periods, under very different climatic conditions; and that all have survived immense changes in the

mention this here, because Mr. Darwin not only directed my earliest studies in the subjects of the distribution and variation of species, but has discussed with me all the arguments, and drawn my attention to many of the facts which I have endeavored to illustrate in this Essay. I know of no other way in which I can acknowledge the extent of my obligation to him, than by adding that I should never have taken up the subject in its present form, but for the advantages I have derived from his friendship and encouragement.

* To Sir Charles Lyell's works, indeed, I am indebted for the enunciation of those principles that are essential to the progress of every naturalist and geologist; those, I mean, that affect the creation and extinction, dispersion and subsequent isolation of organic beings; and though botanists still differ in opinion as to the views he entertains on the most speculative of subjects (the origin and permanence of species), there is, I think, but one as to the soundness and originality of his observations on all that relates to the strict dependence of organic beings on physical conditions in the state of the earth's surface. I feel that I cannot over-estimate the labors of this great philosopher, when I reflect that without them the science of geographical distribution would have been with me little beyond a tabulation of important facts; and that I am indebted to them, not only for having given a direction to my studies in this department, but for an example of admirable reasoning on the facts he has collected regarding the distribution of plants and animals. I have no hesitation in recommending the 'Principles of Geology' to the New Zealand student of Nature, as the most important work he can study.

† See the Principles of Geology, ed. 9, p. 702, and Address to the Geological Society of London by the President (Leonard Horner, Esq.), in 1847, p. 66.

configuration of the land and seas of Northern Europe. The arguments which support this theory are based upon evidence derived from Zoology and Geology,* and they receive additional weight from the fact that the distribution of British plants is in accordance with its principal features.†

The geographical distribution of British plants has been the subject of the most rigorous investigation by one of our ablest British botanists, Mr. H. C. Watson, who first drew attention to the various botanical elements of which the flora is composed, and grouped the species into botanical provinces. These provinces were intended for 'showing the areas of plants, as facts in nature independent of all theoretical explanations and reasons.' (Cybele Britannica, vol. i, p. 18). An inspection of them shows the relations borne by the plants of England to those of certain parts of Europe and of the Arctic regions; and Professor Forbes, applying a modification of these botanical provinces to the illustration of his views of the original introduction of plants into the British Islands, proceeds to show that their migration took place at different periods, contemporary of course with the connection by land of each botanical region of Britain with that part of the continent which presents a similar association of plants.

To extend a theoretical application of these views to the New Zealand Flora, it is necessary to assume that there was at one time a land communication by which the Chilian plants were interchanged; that at the same or another epoch the Australian, at a third the Antarctic, and at a fourth the Pacific floras were added to the assemblage. It is not necessary to suppose that for this interchange there was a continuous connection between any two of these localities, for an intermediate land, peopled with some or all of the plants common to both, may have existed between New Zealand and Chili when neither of these countries was as yet above water.‡ To account, however, for the Antarctic

* For the contents of the Essay itself, I must refer to the Records of the Geological Survey of Great Britain, vol. i, p. 336. This is the most original and able essay that has ever appeared on this subject, and though I cannot subscribe to all its botanical details, I consider that the mode of reasoning adopted is sound, and of universal application. What I dissent from most strongly is, the origin of the gulf-weed, the peopling of Scotch mountains by iceberg transport of seeds, and the too great stress laid upon the west Irish flora, whose peculiarities appear to me to be considerably over-estimated.

† It may be well to state to the New Zealand student, that there are no reasons to suppose that Botany can ever be expected to give that direct proof of plants having survived geological changes of climate, sea, and land, which animals do; the cause is evident, for the bones of quadrupeds, shells of mollusca, and hard parts of many animals, afford an abundant means of specific identification, and such are preserved when the animals perish. In plants the case is widely different: their perishable organs of reproduction, which alone are available for systematic purposes, are seldom imbedded, even when other parts of the plants are.

‡ This disappearance of old land, and the migration of its flora and fauna to new, may be illustrated to a certain extent by the delta of any New Zealand river. A mud-bank on one shore, covered with mangroves, advances across the channel, the mangroves growing on the new land as it forms. The current changes, and the end of the bank (with its mangroves) is cut off, and becomes an island: another change of the river channel fills up that between the islet and the opposite shore, to which it hence becomes a peninsula, peopled by mangroves, whose parents grew on the opposite bank. Here, be it remarked, no subsidence is required, such as must have operated in the assumed isolation of New Zealand.

plants on the lofty mountains, a new set of influences is demanded; no land connection between these islands and New Zealand could have effected this, for the climate of the intermediate area must necessarily have prevented it. But changes of relation between sea and land induce changes of climate, and the presence of a large continent connecting the Antarctic islands would, under certain circumstances render New Zealand as cold as Britain was during the glacial epoch. Sir C. Lyell first demonstrated this, and showed what such conditions should be; and by consulting the 'Principles of Geology,' my reader will understand how such a climate would reign in the latitude of New Zealand, as that its flora should consist of what are now Antarctic forms of vegetation. The retirement of the plants to the summit of the New Zealand mountains,* would be the necessary consequence of the amelioration of climate that followed the isolation of New Zealand, and the replacement of the Antarctic continent by the present ocean.

The climate throughout the south temperate zone is so equable, and the isothermal lines are so parallel to those of latitude, that it is not easy for the New Zealand naturalist to realize the altered circumstances that would render the plains of his island suitable for the growth of plants that now inhabit its mountains only;† but if he glance at the map of the isothermal lines of the northern hemisphere, he will see how varied are the climates of regions in the same latitude; that London, with a mean temperature of 51°, is in the same latitude as Hudson's Bay, where the mean temperature is 30°, and the soil ever frozen: and he will further be able to understand by a little reflection, how a change in the relative positions of sea and land would, by isolating Labrador, raise its temperature 10°–15°, causing the destruction of all the native plants that did not retire to its mountain-tops, and favoring the immigration of the species of a more genial climate.

The first inference from such an hypothesis is that the Alpine plants of New Zealand, having survived the greatest changes, are its most ancient colonists; and it is a most important one in many respects, but especially when considered with reference to the mountain floras of the

* With regard to the British mountains, Professor Forbes imagines that they were islets in the glacial ocean, and received their plants by transportation of seeds with soil, on ice from the Arctic regions. This appears to me to want support, and there is much in the distribution of Arctic plants especially, wholly opposed to the idea of ice transport being an active agent in dispersion. A lowering of 10° of mean temperature would render the greater part of Britain suitable to the growth of Arctic plants; it would give it the climate of Labrador, situated in the same latitude on the opposite side of the Atlantic. Britain is the warmest spot in its latitude, and a very slight geological change would lower its mean temperature many degrees.

† The New Zealand naturalist has probably a very simple means of determining for himself whether his island has been subject to a geologically recent amelioration of climate; to do which, let him examine the fiord-like bays of the west coast of the Middle Island, for evidence of the glaciers which there exist in the mountains having formerly descended lower than they now do. Glaciers to this day descend to the level of the sea in South Chili, at the latitude of Dusky Bay; and if they have done so in the latter locality, they will have left memorials, in the shape of boulders, moraines, and scratched and polished rocks.

Pacific and southern hemisphere generally. These may be classed under three heads:*—

1. Those that contain identical or representative species of the Antarctic Flora, and none that are peculiarly Arctic; as the Tasmanian and New Zealand Alps.†

2. Those that contain, besides these, peculiarities of the Northern and Arctic Floras;‡ as the South American Alps.

3. Those that contain the peculiarities of neither; as the mountains of South Africa and the Pacific Islands.

We thus observe that the want of an Arctic or Antarctic Flora at all in the Pacific islands, and the presence of an Arctic one in the American Alps, are the prominent features; and I shall confine my remarks upon these to the fact that, with regard to the isolated islands of the Pacific, they are situated in too warm a latitude to have had their temperature cooled by changes in the relative position of land and ocean, so as to have harbored an Antarctic vegetation. With regard to the South American Alps, there is direct land communication along the Andes from Arctic to Antarctic regions; by which not only may the strictly Arctic genera and species have migrated to Cape Horn, but by which many Antarctic ones may have advanced northward to the equator.§

There is still another point in connection with the subject of the relative antiquity of plants, and in adducing it I must again refer to the 'Principles of Geology,' where it is said, 'As a general rule, species common to many distant provinces, or those now found to inhabit many distant parts of the globe, are to be regarded as the most ancient .... their wide diffusion shows that they have had a long time to spread themselves, and have been able to survive many important changes in Physical Geography.'‖ If this be true, it follows that, consistently with the theory of the antiquity of the Alpine flora of New Zealand, we should find amongst the plants common to New Zealand and the Antarctic islands, some of the most cosmopolitan; and we do so in *Montia*

* I need scarcely remind my reader that in thus sketching the characteristics of these Alpine floras, I make no allusion to exceptions that do not alter the main features. I am far from asserting that there are no peculiar Arctic or Antarctic forms in the Pacific Islands, nor any peculiarly Arctic ones in Tasmania and New Zealand: but if, on the one hand, future discoveries of such shall weaken the points of difference between these three mountain regions, on the other they might be very much strengthened by adducing the number of Arctic species common to the South American Alps, but not found in the others.

† These Antarctic forms are very numerous; familiar ones are *Acœna, Drapetes, Donatia, Gunnera, Oreomyrrhis, Lagenophora, Forstera, Ourisia, Fagus, Callixene, Astelia, Gaimardia, Alepyrum, Oreobolus, Carpha, Uncinia.*

‡ *Berberis, Sisymbrium, Thlaspi, Arabis, Draba, Sagina, Lychnis, Cerastium, Fragaria, Lathyrus, Vicia, Hippuris, Chrysoplenium, Ribes, Saxifraga, Valeriana, Aster, Hieracium, Stachys, Primula, Anagallis, Pinguicula, Statice, Empetrum, Phleum, Elymus, Hordeum.*

§ Why these Antarctic forms have not extended into North America, as the Arctic ones have into South America, is a curious problem, and the only hypothesis that suggests itself is derived from the fact that though the Panama Andes are not now sufficiently lofty for the transit of either, there is nothing to contradict the supposition that they may have had sufficient altitude at a former period, and that one which preceded the advance of the Antarctic species to so high a northern latitude.

‖ Principles of Geology, ed. 9, p. 702.

*fontana*, *Callitriche verna*, *Cardamine hirsuta*, *Epilobium tetragonum*, and many others.

On the other hand, it must be recollected that there are other causes besides antiquity and facility for migration, that determine the distribution of plants; these are their power, mentioned above, of invading and effecting a settlement in a country preoccupied with its own species, and their adaptability to various climates: with regard to the first of these points, it is of more importance than is generally assumed, and I have alluded to its effects under *Sonchus*, in the body of this work. As regards climates, the plants mentioned above seem wonderfully indifferent to its effects.*

Again, even though we may safely pronounce most species of ubiquitous plants to have outlived many geological changes, we may not reverse the position, and assume local species to be amongst the most recently created; for whether (as has been conjectured) species, like individuals, die out in the course of time, following some inscrutable law whose operations we have not yet traced, or whether (as in some instances we know to be the case) they are destroyed by natural causes (geological or others), they must in either case become scarce and local while they are in process of disappearance.

In the above speculative review of some of the causes which appear to affect the life and range of species in the vegetable kingdom, I have not touched upon one point, namely, that which concerns the original introduction of existing species of plants upon the earth. I have assumed that they have existed for ages in the forms they now retain, that assumption agreeing, in my opinion, with the facts elicited by a survey of all the phenomena they present, and, according to the most eminent zoologists, with those laws that govern animal life also; but there is nothing in what is assumed above, in favor of the antiquity of species and their wide distribution, that is inconsistent with any theory of their origin that the speculator may adopt. My object has not so much been to ascertain what may, or may not, have been the original condition of species, as to show that, granting more scope for variation than is generally allowed, still there are no unassailable grounds for concluding that they now vary so as to obliterate specific character; in other words, I have endeavored to show that they are, for all practical purposes of progress in botanical science, to be regarded as permanently distinct creations, which have survived great geological changes, and which will either die out, or be destroyed, with their distinctive marks unchanged. We have direct evidence of the impoverishment of the flora of the globe, in the extinction of many most peculiar insular species within the last century; but whether the balance of nature is kept up by the consequent increase of the remainder in individuals, or by the sudden creation of new ones, does not appear, nor have we any means of knowing: if the expression of an opinion be insisted on, I should be induced to follow the example of an eminent astronomer,

* Mr. Watson (Cybele Britannica) gives the range of *Callitriche* in Britain alone as including mean temperatures of 40° to 52°, and as ascending from the level of the sea to nearly 2000 feet in the East Highlands of Scotland. *Montia*, according to the same authority, enjoys a range of 36° to 52°, and ascends to 3300 feet; *Epilobium*, a temperature of 40° to 51°, and ascends to 2000 feet; *Cardamine*, a temperature of 37° to 52°, and ascends to 3000 feet.

who, when the question was put to him, as to whether the planets are inhabited, replied that the earth was so, and left his querist to argue from analogy. So with regard to species, we know that they perish suddenly or gradually, without varying into other forms to take their place as species, from which established premiss the speculator may draw his own conclusions. * * * * *

To those who may accuse me of giving way to hasty generalization or loose speculation on the antiquity and dispersion of plants over parts of the Southern Hemisphere, I may answer, that no speculation is idle or fruitless, that is not opposed to truth or to probability, and which, whilst it co-ordinates a body of well established facts, does so without violence to nature, and with a due regard to the possible results of future discoveries. I may add, that after twelve years' devotion to the laborious accumulation and arrangement of facts in the field and closet, untrammelled by any theories to combat or vindicate, I have thought that I might bring forward the conclusions to which my studies have led me, with less chance of incurring such a reproach, than those would, who with far better abilities and judgment, have not had my experience and opportunities."

The hypothesis that these insular floras are the remains of larger floras greatly reduced or verging to extinction, will be found remarkably applicable, if we mistake not to the case of the Sandwich Islands; many of the characteristic species of which appear to be represented by comparatively few individuals, as if waning to extinction along with the Hawaian race.

The general idea here so ably set forth by Dr. Hooker, in respect to the Antarctic and South Pacific floras, is a very important one, and worthy of the most extended and critical examination. Its establishment, moreover, by satisfactory evidence, would destroy one of the strongest grounds upon which the doctrine of the multiple origin of species (at least in the form maintained by Schouw) is supported. To complete the view, and fairly to exhibit the grounds upon which these theoretical conclusions are based, we should embrace in our extracts a large part of the remaining chapter; on the physiognomy and affinities of the New Zealand Flora, and on the variation of New Zealand species. But the necessary limits of our article forbid. A few facts regarding the more striking peculiarities of the New Zealand flora may be collected. The large proportion, both relatively and absolutely, of the Cryptogamia, and especially of Ferns, has already been noted. "A paucity of Grasses, and absence of Leguminosæ, and abundance of bushes and Ferns, and a want of annual plants, are the prevalent features of the open country; whilst the forests abound in Cryptogamia, in Phænogamic plants with obscure and green flowers, and very often of obscure and little known natural orders."

The number of natural orders of the Phænogamic plants is remarkably large in proportion to the genera, even for an insular flora; being 92 to 282, or about one to three: while the genera

are to the species as 282 to 730, each genus having on the average only 2½ species: so that the species average but eight for each order. This makes it one of the most difficult floras in the world for a beginner, who must know a natural order for every eight species. How recondite, vague, and unsatisfactory the natural system of Botany must appear to the New Zealand student!

Of the largest natural orders, as respects the number of species, the individuals are often so few that none of them form prominent features in the landscape. The *Coniferæ* prove to be the most prevalent family; but the majority of their species are not social but grow intermixed with the trees, so as to give no character to the landscape;—a case just opposite to what occurs in the northern hemisphere, at least in North America, where the species are few, but mostly social, and existing in a vast number of individuals, which often occupy considerable tracts almost exclusively, and thus strikingly impress their features upon the landscape.

The number of kinds of trees is very large in proportion to the herbaceous plants; as there are 113 Phænogamous trees, including shrubs above twenty feet high, or one-sixth of the flora, while in England there are not more than 35 native trees, in a much larger flora.

A remarkably large proportion of the Phænogamous flora of New Zealand consists of absolutely peculiar plants: of these there are 26 genera and 507 species; or more than two-thirds of the whole. The greater part of these are Exogens. Of the remaining third, consisting of plants common to New Zealand and other countries:

"193 species, or nearly one-fourth of the whole, are Australian.
89 species, or nearly one-eighth of the whole, are South American.
77 species, or nearly one-tenth of the whole, are common to both the above.
60 species, or nearly one-twelvth of the whole, are European.
50 species, or nearly one-sixteenth of the whole, are Antarctic Islands, Fuegian, &c."

These several elements in the New Zealand flora, whether as represented by identical or cognate species, are in turn subjected to a critical analysis. The following extract, respecting the Australian element, is interesting from its direct bearing on the question of transport by water.

"If the number of plants common to Australia and New Zealand is great, and quite unaccountable for by transport, the absence of certain very extensive groups of the former country is still more incompatible with the theory of extensive migration by oceanic or aerial currents. This absence is most conspicuous in the case of *Eucalypti*, and almost every other genus of *Myrtaceæ*, of the whole immense genus of ***Acacia***,

and of its numerous Australian congeners, with the single exception of *Clianthus*, of which there are but two known species, one in Australia, and the other in New Zealand and Norfolk Island.

The rarity of *Proteaceæ*, *Rutaceæ*, and *Stylideæ*, and the absence of *Casuarina* and *Callitris*, of any *Goodeniæ* but *G. littoralis* (equally found in South America), of *Tremandreæ*, *Dilleniaceæ*, and of various genera of *Monocotyledones*, admit of no explanation consistent with migration over water having introduced more than a very few of the plants common to these tracts of land. Considering that *Eucalypti* form the most prevalent forest feature over the greater part of South and East Australia, rivalled by the *Leguminosæ* alone, and that both these Orders (the latter especially) are admirably adapted constitutionally for transport, and that the species are not particularly local or scarce, and grow well wherever sown, the fact of their absence from New Zealand cannot be too strongly pressed on the attention of the botanical geographer; for it is the main cause of the difference between the floras of these two great masses of land being much greater than that between any two equally large contiguous ones on the face of the globe. If no theory of transport will account for these facts, still less will any of variation; for of the three genera of *Leguminosæ* which do inhabit New Zealand, none favor such a theory; one, *Clianthus*, I have just mentioned; the second, *Edwardsia*, consists of one tree, identical with a Juan Fernandez and Chilian one, and unknown in New Holland; and the third genus *(Carmichælia)* is quite peculiar, and consists of a few species feebly allied to some New Holland plants, but exceedingly different in structure from any of that extensive Natural Order."

Dr. Hooker then appends a carefully prepared table of 228 phænogamous species which may be said to represent each other in two or all the three South temperate masses of land, viz., New Zealand (including Auckland and Campbell's Islands), Australia (including Tasmania), and extra-tropical South America, including the Falkland Islands; the list being confined to cases of real and usually very close botanical affinity, to the exclusion of analogical resemblances, however striking. The list is by no means overstrained, nor as full as it might be; since one or two more good instances have occurred to our memory while looking over its columns. On comparing together the Australian and New Zealand columns, we find only fourteen blanks, not filled by known representative species; a similar comparison of the New Zealand and South American columns shows forty-six blanks, sixteen of which are among the Endogens.

On fairly weighing all this testimony, the botanist will perhaps accede to our author's conclusion,—viz., that the floras of these three great areas of land in southern latitudes "exhibit a botanical relationship as strong as that which prevails throughout the lands within the Arctic northern temperate zones: and which is not to be accounted for by any theory of transport or of variation; but which is agreeable to the hypothesis of all being members of a once more extensive flora, which has been broken up by geological and climatic causes." A. G.

ART. XXXVII.—*Remarks on the Mineral species Algerite;* by T. S. HUNT.

IN this Journal for July, 1849, I published a description by Mr. Alger, of a mineral from Franklin, N. J., together with an analysis of it by myself, from which I was led to consider it a new species, and to give it the name of Algerite. It was again analyzed by Mr. R. Crossley, whose results will be found in this Journal for July, 1850. The mineral had been described as having the form of an oblique rhombic prism, $M:M = 94°$, but Mr. Dana has satisfied himself that the crystal is really a square prism. The hardness is 3·0—3·5 (Alger, Crossley); density 2·697—2·712 (Hunt), 2·78 (Crossley). The mineral which is imbedded in a white coarse-grained crystalline limestone, is subject to decomposition in the weathered portions of the rock, but as I stated in my paper, "the crystals selected for analysis were hard, semi-translucent and undecomposed; their powder when elutriated and carefully dried, was of a buff color, which was not changed by ignition." Mr. Crossley remarks that in his case "many of the crystals were incrusted with idocrase, and in some instances penetrated so much that it required great caution to secure portions free from that mineral." He rejected all decomposed portions, and analyzed only pure honey-yellow fragments. The results of the two analyses are as follows:

| | HUNT. | CROSSLEY. |
|---|---|---|
| Silica, | 49·82 | 49·96 |
| Alumina, | 24·91 | 24·41 |
| Peroxyd of iron, | 1·85 | 1·48 |
| Magnesia, | 1·15 | 5·18 |
| Potash, Soda, | 10·21 | 9·97 |
| Carbonate of lime, | 3·94 | 4·21 |
| Water, | 7·57 | 5·06 |
| | 99·45 | 100·27 |

A subsequent incomplete analysis gave me, Silica 49·42, alumina 25·67, peroxyd of iron 1·93, carbonate of lime 3·57, water 6·18, magnesia and alkalies by difference 13·23. My analysis differs from that of Mr. Crossley in the smaller amount of magnesia which is replaced by water, a case analogous to that of aspasiolite and iolite, or serpentine and chrysolite, which are found in the same localities and even in the same crystal. The question whether these differences are to be attributed to a subsequent alteration of the minerals, by removing magnesia and substituting water, or to the original association of homœomorphous, chemically homologous species, is as yet unsettled. The latter view is maintained by Scheerer, whose opinions will be found in this Journal [2] vols. v, p. 388, and vi, p. 199; and also cited with some further considerations to the same effect, in a paper by myself, published in the last volume of this Journal, of which see page 217.

From the square form of the prism Mr. Dana has suggested that Algerite may be a result of the alteration of scapolite from which species however it differs widely in composition; while the latter affords from 13 to 24 p. c. of lime, and from 4 to 7 of soda, Algerite contains neither lime nor soda, but 10 p. c. of potash, so that the change must have consisted in the removal of lime and soda, and the substitution of magnesia, water, and a large amount of potash, which reactions, especially the replacement of one alkali by another, seem very questionable. As scapolite has not yet been found with this mineral, it would have been better to regard algerite as derived from the alteration of the associated idocrase, a process which presents less difficulty to the chemist than the other. It may fairly be questioned whether the extreme views maintained by some, with regard to the alteration of mineral species, do not tend to discourage mineralogical chemistry, and to embarrass the science with groundless hypotheses.

In support of the view that Algerite is an altered scapolite, Mr. J. D. Whitney has given at page 296 of this volume, the results of his examination of the mineral. He observes of the material selected for analysis, that "after the ignition it was noticed that portions of the ignited mineral remained nearly unaltered in appearance, while the larger part acquired a brick-red color, and on examination with the microscope was seen to contain small silvery scales apparently of mica. As only a small quantity could be used for analysis, the results can be relied upon only as approximatively correct." p. 208. He gives the following as the result of his analysis: Silica 52·09, alumina 18·63, phosphate of lime 8·22, carbonate of lime 4·41, water 6·68, loss, potash and soda? 9·97 = 100·00.

Had the analysis of such a compound agreed with my own, I might have had reason to doubt the results of Mr. Crossley and myself. Since reading Mr. Whitney's remarks I have examined several crystals of Algerite for phosphate of lime, by digesting their powder with heated hydrochloric acid, and testing the solution with molybdate of ammonia; this delicate reagent indicated the presence of traces only of phosphoric acid. The apparently pure calcite in which the crystals are imbedded, gives a more decided reaction, and traces of this acid are seldom wanting even in feldspars and tourmalines. While, therefore, we do not question that Mr. Whitney's parti-colored mixture may have contained apatite as well as mica, we find in his confessedly approximate results, no reason for doubting that Algerite is a homogeneous mineral, or for supposing it to be produced by the alteration of scapolite. Further examinations of the mineral may throw more light upon the variations in the proportions of water and magnesia.

Montreal, C. E., March 7th, 1854.

ART. XXXVIII.—*Notice of a collection of Fishes from the southern bend of the Tennessee River, in the State of Alabama;* by L. AGASSIZ.

(Concluded from page 308.)

CYPRINODONTS, *Agass.*—Only two species of this family have thus far been discovered in the waters of the Tennessee River, and both of them have already been described by Dr. Storer under the names of *Pœcilia catenata* and *olivacea*, Synopsis, p. 178. Having made lately however, a thorough revision of the genera and species of this family found in the United States, I would remark that *Pœcilia catenata*, St., ought to be referred to the genus *Hydrargyra*, and that *Pœcilia olivacea* belongs to my newly established genus *Zygonectes*. These species ought therefore to stand in future in our systematic catalogues under the names of *Hydrargyra catenata*, and *Zygonectes olivaceus*.*

CYPRINOIDS, *Cuv.*—This is one of the most interesting families of our freshwater fishes, both on account of the number of genera and species inhabiting our lakes and rivers, and of the diversity of their forms and habits.

CARPIODES, *Rafin.*—In the great French Ichthyology, Valenciennes has established a new genus under the name of *Sclerognathus*, for Lesueur's *Catostomus cyprinus*, and this genus has deservedly been acknowledged by subsequent writers. In con-

* The species of the genus Zygonectes may be arranged in two groups: 1, those in which there are several more or less distinctly dotted lines along the sides of the body, and in which a broad black band extends across the eye and cheek. To this group belong: *Z. Nottii*, Agass. The darker continuous longitudinal lines alternate with fainter interrupted ones. Males with distinct, transverse bands. Dark olive above, fading upon the sides, silvery below. Operculum, throat, and space in advance of the eye light orange color. Mobile, Alabama. Collected there with Dr. Nott. Mississippi: Col. Deas.—*Z. lineolatus*, Agass. Longitudinal lines broader and undulated or serrated, the transverse bands of the male very distinct and broader than the longitudinal ones. Olive colored, darker along the back and fading upon the sides, lower parts silvery Discovered by Dr. W. I Burnett at Augusta, Ga.—*Z. guttatus*, Agass. A large dark spot upon the centre of each scale on the back and sides, forming longitudinal rows of disconnected dots. The transverse bars of the males are much narrower and nearer together than in Z. lineolatus. Dark olive above, fading upon the sides; abdomen silvery. Mobile, Alabama.—*Z. dispar*, Agass. Longitudinal lines of minute dots particularly distinct in the anterior part of the body, alternating backwards with continuous lines in the males, which are besides transversely barred, whilst the female has only continuous serrated lines upon the sides. Light olive above, silvery upon the sides and below. In small creeks near St. Louis, Mo., on the Illinois side of the Mississippi, and also in the Illinois River at Beardstown.—*Z. hieroglyphicus*, Agass. Anterior and upper part of the body irregularly sprinkled with dark spots, passing into longitudinal rows backwards. Light olive above, silvery upon the sides and below. Mobile, Alabama. 2. The second group includes species with one broad longitudinal black band extending from the tip of the lower jaw to the base of the tail, passing in a straight line through the eyes and along the sides of the body. To this group belongs the species mentioned above from the Tennessee River, and also *Z. lateralis*, Agass, which is a more elongated species from Mobile, Alabama; also dotted above the broad lateral band, and *Z. zonatus*, Agass., from St. Louis, Mo., which has no spots upon the sides of the back, and in which the outlines of the longitudinal band are serrated.

sidering this type of Cyprinoids as a distinct group among the Catostomi, Rafinesque has however the priority over the able professor of the Jardin des Plantes; for we find in his *Ichthyologia Ohiensis* that the third subgenus of Catostomus, which he calls *Carpiodes*, though not characterized with the precision with which Valenciennes has circumscribed his genus Sclerognathus, exactly corresponds to it. I do not hesitate therefore to adopt Rafinesque's name as the older; the more so, since this writer has at the same time wisely separated from the common Catostomi at that early day two other types of the same group, which are even now left among Catostomi by all ichthyologists. I allude to the subgenus *Ictiobus*, with *Catostomus Bubalus* as its type, and to the genus *Cycleptus* for the *Missouri sucker;* for though Rafinesque did not himself examine this latter fish, and ascribes to it two dorsals, it must be evident to any one who has had an opportunity of investigating this rare species that the few words with which it is mentioned apply to it, and that the indication of two dorsals is easily explained by the very form of that fin, the anterior part of which rises like a separate fin in advance of the following low part which extends uniformly far behind. I should add that *Catostomus elongatus* belongs also to this genus *Cycleptus*. As to *Ictiobus*, it resembles Carpiodes in external appearance, but is at once distinguished by its thin lips and more terminal mouth.* Nothing is to be more regretted for the progress of Natural History in this country, than that Rafinesque did not put up somewhere a collection of all the genera and species he has established, with well authenticated labels, or that his cotemporaries did not follow in his steps, or at least preserve the tradition of his doings, instead of decrying him and appealing to foreign authority against him. Tracing his course as a naturalist during his residence in this country, it is plain that he alarmed those with whom he had intercourse by his innovations and that they preferred to lean upon the authority of the great naturalists of the age then residing in Europe, who however knew little of the special Natural History of this country, than to trust the somewhat hasty man who was living among them, and who had collected a vast amount of information from all parts of the States, upon a variety of objects then entirely new to science. From what I can learn of Rafinesque, and from a careful study

* In connection with the genera mentioned above, I may remark here that Rafinesque has established another sub-genus under the name of *Moxostoma*, which fully deserves to be recognized as a distinct genus, as far as I am able to judge from the three species belonging to it, with which I am especially acquainted, which are *Catostomus anisurus* of the West, *C. gibbosus* or *tuberculatus* of the East, and *C. Suceti* of the South. After acknowledging these alterations of the genus Catostomus, as it is now generally understood by ichthyologists, there would still remain a group of species to constitute the genus Catostomus proper of which *C. hudsonius*, for which the name Catostomus was first proposed, may be considered as the type. Thus freed of all unjustifiable additions engrafted upon it in course of time, the genus Catostomus would be restored to its primitive natural circumscription.

of his works, I am satisfied that he was a better man than he appeared. His misfortune was his prurient desire for novelties, and his rashness in publishing them, and yet both in Europe and in America he has anticipated most of his cotemporaries in the discovery of new genera and species in those departments of science which he has cultivated most perseveringly, and it is but justice to restore them to him, whenever it can be done. Personal considerations should no longer be allowed to interfere with this late act of redress. May the example of Rafinesque not be lost for those naturalists in this country who describe new species without taking the least care to preserve the original specimens of their descriptions, or to circulate authentic ones among other naturalists.

Besides the well known type of the genus Carpiodes, the *C. Cyprinus*, and the other species described by Valenciennes and Rafinesque, I have ascertained the existence of five undescribed species, of which I give below short comparative descriptions. These species bear to one another similar relations as the species of Cyprinus described by Heckel; indeed they truly represent upon the Continent of North America the genus Cyprinus of the old world to which they bear the greatest resemblance in outward appearance, though they differ strikingly in their generic characters. I have applied to the new species here mentioned names reminding us of the common name of Buffalo applied to all of them throughout the country. The large number of specimens including all sizes, which I have been able to collect of some species of this genus, has enabled me to ascertain the range of variation in their characters.

1. *Carpiodes Urus*, Agass.—From the Tennessee River. It grows very large, weighing occasionally from 30 to 40 pounds. The body in this species is not so high as in C. Cyprinus, nor is it so compressed above; the scales are also not so high, but more angular behind, and the anterior portion of the dorsal is not so elongated. The gill cover is larger, and the distance from the hind border of the eye to the inferior angle of the subopercle, near the base of the pectorals, and the distance from the same point to the superior and posterior angle of the opercle, are nearly equal. In C. Cyprinus the distances differ by nearly one-third. The subopercle is not triangular, but its hind border is nearly regularly arched from the upper angle to the posterior angle of the interopercle. The anal has its posterior margin full, and not lunate; the caudal is not so deeply furcate as in C. Cyprinus. The ventrals do not reach the anal. All fins are of a dark color. I am indebted to Dr. Newman for this species.

2. *Carpiodes Taurus*, Agass.—From Mobile River, Alabama. The form of the body is intermediate between that of C. Cyprinus and C. Urus. The gill cover has the same form as in

C. Urus, but it is larger and more strongly arched behind. The hind margin of the scales is waving, owing to a somewhat prominent middle angle. The anterior rays of the dorsal equal in length two-thirds of that of the base of the fin. Anal not lunate behind. The ventrals do not reach to the anal opening. Caudal not so deeply furcate as in C. Cyprinus.

3. *Carpiodes Bison*, Agass.—From the Osage River, Missouri. This species is more elongated than C. Taurus. The head is smaller, the opercle also smaller, and the subopercle triangular. The dorsal has its anterior rays longer, hence its hinder border is more deeply emarginate. Anal more deeply lunate. Horizontal diameter of scales greater. I have received this species through the attention of Mr. George Stolley.

4. *Carpiodes Vitulus*, Agass.—From the Wabash River, Indiana. This seems to be a smaller species than the preceding ones. The form of the body resembles that of C. Taurus; but the eyes are smaller; the opercle is more broadly rounded behind; the subopercle has its posterior and free border regularly arched above and below, and not emarginate as in C. Taurus. The direction of the numerous water tubes on the head and cheeks also differ. The upper and lower borders of the scales are nearly straight. The dorsal does not extend quite so far forwards. I am indebted to Col. Richard Owen of New Harmony for this species.

5. *Carpiodes Vacca*, Agass.—From the Susquehannah River. This species resembles more closely C. Cyprinus than any other; the anterior rays of the dorsal are also very elongated, yet they do not reach beyond the base of the fin itself when bent backwards; the caudal is not so deeply furcate, and the scales have a greater horizontal diameter. I owe this species to the kindness of Professor S. S. Haldeman.

Catostomus, *Lesueur*.—The following species of this genus have been collected by Dr. Newman in the vicinity of Huntsville:

*Catostomus communis*, Lesueur.—Called Fine-scaled Sucker at Huntsville.

*Catostomus nigricans*, Lesueur.—Called Hog Sucker at Huntsville.

*Catostomus Duquesnii*, Lesueur.—Called May Sucker at Huntsville.

*Catostomus melanops*, Kirtl.—Also called May Sucker at Huntsville. This species agrees with Kirtland's description of C. melanops, except in having longer pectorals and in the reddish color along the sides. Rafinesque's description cannot apply to this fish. Having no specimens from the localities mentioned by Rafinesque and Kirtland, I do not venture to pursue further a comparison between these fishes.

Rhinichthys, *Agass.*—This genus was established by me in "Lake Superior," page 353. Several new species have been dis-

covered since by Prof. Baird and myself.* Dr. Newman has sent me another undescribed species, which I call

*Rhinichthys obtusus*, Agass.—Body cylindrical, slightly compressed, more blunt than in Rh. marmoratus. The mouth extends but little beyond the margin of the upper jaw; lower jaw strongly arched from side to side. Eyes rather large and nearer the end of the snout than the posterior angle of the opercle. Dorsal exactly intermediate between the ventrals and the anal, quadrangular, its last rays about two-thirds the length of the fish, so that when the fin is folded backwards their ends meet. Pectorals broadly rounded behind; do not reach the base of the ventrals. Caudal not very deeply furcate; its lobes are broad, rather than slender, the lower lobe is generally a little the longer. The color of the body is dark chocolate above, and of a silvery white below; these two colors are separated by a longitudinal band of a darker color than the back, extending from the end of the snout through the eye in a direct line along the sides to the middle of the base of the caudal. The whole dorsal region is mottled with black blotches, sometimes running together and forming large patches, and often descending to the lighter portion of the sides. Scales rather small. Called Minnow at Huntsville. Found in the Spring branch.

Chondrostoma, *Agass.*—This genus was established by me in 1834 in the Mémoires de la Société des Sc. Nat. de Neuchâtel, for the Cyprinus Nasus of Europe, and has been adopted with various modifications by subsequent writers. Thus far no representative of this type had been known to exist in North America, though the species I now refer to it here, has been described for sometime by Dr. D. H. Storer; but having been referred to the genus Leuciscus, to which C. Nasus was also referred formerly, it has not been distinguished from the ordinary Leucisci. I need only allude to it for the present.† Other species occur in the fresh waters of the Pacific coast of North America. *Exoglossum dubium*, Kirtl., may belong to this genus.

*Chondrostoma prolixum*, Agass.—Leuciscus prolixus, *Storer*, Synops., page 165. Called Minnow at Huntsville. Found in the Spring branch.

* I am indebted for another new species of this genus to Dr. I. H. Rauch, of Burlington, Iowa, which I would call *R. Meleagris*, Ag. It is remarkably short and stout in comparison to its congeneric types, also smaller. The whole body is dotted with black upon a silvery ground, the dots partly confluent; the belly only is plain silvery white.

† I owe another entirely new species of this genus to Dr. I. H. Rauch, of Burlington, Iowa; which I inscribe as *Ch. pullum*, Ag. It is the smallest species of the genus; much broader than the others in comparison to its length; head especially small, almost indicating distinct generic peculiarities, into which I am however unable to enquire from want of a sufficient number of specimens. This pretty little fish is of a peculiar deep but dull green, darker above, passing into yellowish white below.

Hybopsis,* *Agass.*—So little attention has thus far been paid to the generic differences existing between the American Cyprinoids that it is not surprising to find several yet unnoticed. Among others I mention here a new type remarkable for its slender elongated form, its long head, its obtuse prominent snout, its inferior mouth and the advanced position of the anal. This genus is founded upon a small species from Huntsville. *Leuciscus Storerianus*, Kirtland, which I have however not examined in nature, may be another species.

*Hybopsis gracilis*, Agass.—Body much elongated and slightly compressed; head long, equalling nearly one-fourth the entire length of the fish. The snout is very short and broadly rounded; the nostrils are large, above the middle line of the eye and nearer the end of the snout than the centre of the eye. The eyes are very large in proportion to the size and width of the head; the horizontal diameter which is slightly the longest, equals one-third the length of the head, their upper edge is on a line with the top of the head, the lower edge with the anterior edge of the intermaxillaries and the extremity of the upper maxillaries reaches the line of their anterior border. The fins are all long and pointed. The pectorals are low down on the sides and reach the base of the ventrals. The hinder base of the dorsal is midway between the end of the snout and the extremity of the tail. The height of the dorsal is one-third greater than the length of the base; the second and the third rays longest; number total of rays 8, and two united as one for the last ray of the fin. The base of the ventrals is below the anterior part of the dorsal; their extremities reach nearly to the anal fin. The distance of the anal from the base of the tail is equal to twice the length of its own base. The anal is like the dorsal in form, but smaller, number of rays 7, with a last double ray. Caudal long, deeply furcate, the lobes being slender and pointed.

Chrosomus, *Rafin.*—The fish for which Rafinesque established this group in his genus Luxilus, well deserves to be considered as

* While these pages were setting in type, I have received another pretty species of this interesting genus, through the attention of Dr. I. H. Rauch, from Burlington, Iowa. The large number of specimens obtained enables me to make some additions to the characteristics of the genus: "The mouth is protractile downwards, after the fashion of Catostomus, so much so that had I not had ample opportunity to examine young Catostomi, and to study the changes they undergo with age, I might have supposed my Hybopsis to be the young of some species of that genus. Moreover the lips are not swollen nor thickened. The pharyngeal teeth differ also greatly from those of Catostomi, there being only four or five compressed and hooked ones in each main row, and one or two in a second row."

This new species differs from that of Huntsville, by its smaller size, its more pointed snout and the peculiar coloration. A deep black narrow band extends from the neck to the base of the caudal along the whole back, dividing in advance of the dorsal to encircle that fin, and uniting again behind it upon the middle line. General color olive, silvery upon the sides, the dorsal and caudal faintly tinged with rose color and a deeper rose-colored spot upon the base of the first ray of the dorsal. I shall call this species *H. dorsalis*, Ag.

a distinct genus, as it stands very isolated among the other American Cyprinoids. It may be considered as corresponding upon this continent to the genus *Phoxinus* of Europe, from which it differs however by the continuous lateral line and the shorter lower jaw. Rafinesque has given it a very appropriate specific name, calling it

*Chrosomus erythrogaster*, Raf.—It is one of the prettiest fresh-water fishes of North America, varying greatly with age and at different periods of the year. It remains yet to be ascertained whether the specimens from the Tennessee River are strictly identical with those from the Ohio River. I have received specimens from the Osage River, from Mr. G. Stolley, which differ somewhat in having deeper colors and a somewhat elongated form.

Stilbe, *DeKay*.—In his Natural History of New York, DeKay has established this genus for the Cyprinus chrysoleucos of Mitchell. Without a thorough revision of the many new genera of Cyprinoids established by Heckel and Prince Canino, for which I have not the necessary materials on hand, I am unable to decide whether DeKay's genus may stand or not. So much however is certain, that Storer's *Leuciscus obesus* from Florence, Alabama, which has also been obtained in the vicinity of Huntsville by Dr. Newman, also belongs to this genus. *Abramis versicolor*, DeKay, must also probably be referred to it. I know several other undescribed species of this type from other parts of the United States. It is intermediate between Alburnus and Abramis, having the form of Abramis elongatus, and other elongated species of that genus with comparatively small anal, and the prominent lower jaw of Alburnus.

*Stilbe obesus*, Agass.—Leuciscus obesus, *Storer*, Synopsis, p. 166. Called Hickory or Gizzard Shad at Huntsville.

Hypsolepis, *Baird*.—This genus was established for those species of Leuciscus the body of which is compressed and covered with high short scales. *Leuciscus cornutus* may be considered as its type. My *Leuciscus frontalis* from Lake Superior, is another species of this genus. To it belongs also Dr. Storer's *Leuciscus gibbosus* from Florence, Alabama, which has also been found about Huntsville, by Dr. Newman.

*Hypsolepis gibbosus*, Agass.—Leuciscus gibbosus, *Storer*, Synop., page 166. Called Silver-sides at Huntsville.

Leuciscus, *Cuv*.—One species from Huntsville, the same which Dr. Storer has described from Florence, Alabama, under the name of

*Leuciscus croceus*, Stor., Synop., p. 165.

SAUROIDS, *Agass.*—Before I began to collect the materials for a monograph of the genus Lepidosteus, I had no idea of the wide geographical range of this type in North America. Indeed our ichthyological works mention only Lake Huron, Lake Erie and Lake Champlain in the North, the Ohio and Mississippi in the West, and S. Carolina and Florida in the South, as its home, and the whole number of species described, even including all those of Rafinesque without questioning the validity of any of them, does not exceed nine or ten. Yet I have now, in my own collection, not less than twenty-two well characterized species of the genus, and I have ascertained its existence in all the water systems of the South from Florida to Texas, in the Mississippi and all its larger tributaries up to the latitude of Lake Superior, where it does not however occur, in all the lower great Canadian Lakes, and in the St. Lawrence. Also in those river and Lakes of western New York which empty into the waters of the St. Lawrence; in those of western Pennsylvania emptying into the Ohio, and in all the Atlantic rivers, from the Chesapeake Bay to Florida; leaving only the New England States East of Lake Champlain without any of its representatives. Poey describes also one species from Cuba. It seems however to be wanting west of the Rocky Mountains and in Central America. The species sent me by Dr. Newman from Huntsville, agrees with Rafinesque's

*Lepidosteus platostomus.*—It differs however from the species described under the same name by DeKay from Florida, the original specimen of which I have examined myself. Its name at Huntsville is *Gar.*

The identification of species in this genus is extremely difficult owing to the great changes they undergo with age. Indeed the young differ so much in form and structure from the adult that Rafinesque has established a distinct genus for the young of his *Lepidosteus oxyurus* under the name of *Sarchirus vittatus.* In this immature state these fishes have the upper region of the caudal separate from the lower, as a distinct lobe, the body is scaleless and the pectorals consist of a membrane rising from a fleshy tubercle, hence the name Sarchirus of Rafinesque. Another peculiarity of the young lies in their coloration; they having mostly a broad longitudinal black band along the middle line. This has for a time led Rev. Z. Thompson to consider the young of Richardson's *Lepidosteus huronensis* as a distinct species which he has described as *L. lineatus.* DeKay's *L. Bison* is also the same species as L. huronensis; this differs however widely from the southern *L. osseus* and from Rafinesque's *L. oxyurus* from the Ohio River. I shall take an early opportunity of describing all the species I know of this genus and settling as far as possible their complicated synonymy.

CŒLACANTHS, *Agass.*—Until an extensive and minute comparison of all the representatives of the genus Amia from different parts of the United States can be made to ascertain the true value upon which the different species described by Richardson, DeKay and Valenciennes, are founded, it may be sufficient to mention here the existence of that genus in the waters of the Tennessee under the name of

*Amia calva*, L, which has long been considered and may in reality be the only one of the genus. It is known at Huntsville under the name of Scaly Cat and Carp. Found in Mill ponds.

SILUROIDS, *Cuv.*—Two species of this very natural family have been sent to me from Huntsville by Dr. Newman.

*Pimelodus cœrulescens*, Rafin.—Channel Cat. Grows very large and weighs occasionally over one hundred pounds.

*Pimelodus Catus*, Lin.—Several species are confounded under this name; but it is impossible to characterize them without entering into details which would be out of place in this short notice. Called Mud Cat at Huntsville.

STURIONES, *Cuv.*—Two species of Sturgeons occur in the Tennessee, specimens of which I have received from Dr. Newman.

*Acipenser rubricundus*, Lesueur.

*Acipenser maculosus*, Lesueur.

These two species have been considered as synonymous by some ichthyologists. It is true that the young A. rubicundus like all young Sturgeons are more or less maculate, and yet there are so many other differences between the two specimens I have before me, which are nearly of the same size, that I can hardly consider them as identical. The whole genus requires a thorough revision and would be an interesting subject for a monograph.

There are some genera of North American fresh-water fishes the absence of which surprises me in the collection sent by Dr. Newman, and mention them with the view of calling attention to them more particularly. *Lucioperca*, generally called *Salmon* in the West. Is it not possible that the specimens of Hyostoma described above were mistaken for young Lucioperca and sent as specimens of the Salmon? *Labrax*, known everywhere as White Perch. The presence of the genus Perca seems more doubtful. *Chatœssus*, generally known as Hickory or Gizzard Shad. I fancy that the *Stilbe obesus* mentioned above, was mistaken for a small specimen of this type. *Hyodon*, known as Toothed Herring. *Anguilla*, the Eel. *Lota*, known as Barbot or Eelpout. The genus *Pogostoma*, of Rafinesque, is evidently synonymous with Lota. *Polyodon*, known as Shovelbill, and *Petromyzon*, the Lamper-eel. I should also expect a long-billed

species of Lepidosteus, for the two types of this genus occur everywhere together in the West.

If the study of the geographical distribution of animals is ever to furnish us any indications respecting the circumstances under which organized beings were created, we must, in investigating it, turn our attention particularly to those facts which disclose differences of structure in connection with the special localization of the different representatives of each family within their natural boundaries. For years I have been collecting diligently all the data within my reach bearing upon this question, and from the results of this enquiry already in my possession, I am satisfied that the day is not far distant when we shall know with sufficient precision *where* all the living beings now existing upon earth have made their first appearance. This must of course be the first step towards a deeper insight into the *conditions* of that origin itself.

In connection with this train of thoughts it is interesting to notice how much different families of animals vary from each other in the most prominent features of their geographical distribution. There are those the representatives of which are almost uniformally distributed over the whole range of their natural arena. Such is the family of Salmonidæ proper. There are species of true Salmo, of Thymallus, of Coregonus, of Osmerus very equally scattered over Europe, Asia and North America. The same is the case with the family of Esoces, which has however a much greater number of species in the fresh waters of North America. So are also the Sturgeons, with this difference, that upon the continent of America two peculiar genera, Scaphirhynchus and Polyodon, are added, which have no representatives in the old world. The Percoids however present very different combinations: some types are common to North America, Europe and Northern Asia, as the genera Perca, Lucioperca and Labrax, with this difference however, that North America has many fresh water representatives of the genus Labrax which are wanting in the old world; other types are only to be found either in North America or in the old world,—for instance Grystes, Centrarchus, Pomoxis, Amploplites, Calliurus, Pomotis, have no representatives in Europe where we find in their stead the genera Aspro and Acerina; the balance being in favor of North America as far as the number and diversity of the fresh-water types of this family is concerned, whilst the old world has many more and more diversified marine representatives. The family of Cyprinoids agrees with that of the Percoids in the features of its geographical distribution; the types peculiar to each side of the Atlantic being however more equally distributed, for whilst in the old world we find the genera Cyprinus, Barbus, Tinca, Cobitis, Pelecus, Aspius, Rhodeus, Phoxinus, North America has its Car-

piodes, Ictiobus, Cycleptus, Catostomus, Rhinichthys, Chrosomus, Hypsolepis, Hybopsis which are foreign to the old world, and they share together the genera Alburnus, Chondrostoma, Leuciscus, &c., still, with this difference, that the true Leucisci are far more numerous in the old world than in North America. In the family of Cyprinodonts we find exactly the reverse, there being in North America a much greater diversity and a larger number of representatives of this type than in the old world. The case is still different with the family of the Etheostomoids; which are altogether peculiar to North America, not a single species being known in the old world. The family of Cœlacanths is also entirely foreign to the old world, whilst the Sauroids are represented by one genus, Polypterus in the old world and by another, Lepidosteus in America. The Sciænoids differ in another respect: whilst these fishes inhabit exclusively the sea in the old world, there are in North America besides many marine representatives, a number of fresh-water species constituting a distinct genus, Amblodon. Again the family of Siluroids, is represented by a great variety of species in North America, and only by a few in the old world. Similar facts might be mentioned of other families, but this may be sufficient to show how important it is to combine the study of the modifications of the structure of animals with that of their geographical distribution.

For it is not the presence here or there of this or that species of any genus, or family or higher group which I would particularly consider in the study of the geographical distribution of organized beings, but the localization upon certain parts of the surface of the globe of special modifications of definite types representing each a distinct idea, expressed in a variety of living forms and combined in various ways in time and space.

There is another point of view of equal interest in this connection; the mode of association of different families in different parts of the world. It is a fact for instance that the Goniodonts are limited to South America, and that this family, which is entirely wanting in the old world, has no nearer relative than that genus of Sturgeons peculiar to North America, the Scaphirhynchus. Again, whilst the families mentioned above as characteristic of the North American fresh-water fish fauna seem to be equally distributed over the surface of this vast continent, there is yet a special adaptation of some of their types to peculiar localities. The great similarity of their representatives throughout the Southern Atlantic States, the Gulf States and the Mississippi Valley, as high up as the Ohio, including even Lake Champlain, does not extend to the New England States, which although encircled by this uniform combination of fresh-water animals, have another zoological character, peculiar to itself, and approximating more to that of the old world under the same climatic conditions

than the western and southern parts of the Union. In this isolated region of North America, in this zoological island of New England, as we may well call it, we find neither Lepidosteus, nor Amia, nor Polyodon, nor Amblodon, nor Grystes, nor Centrarchus, nor Pomoxis, nor Ambloplites, nor Calliurus, nor Carpiodes, nor Hyodon, nor indeed any of the characteristic forms of North American fresh-water fishes, so common everywhere else, with the exception of two Pomotis, one Boleosoma, and a few Catostomus. The study of these features is of the greatest importance, inasmuch as it may eventually lead to a better understanding of the intentions implied in this seemingly arbitrary distribution of animal life.

Before closing this notice I would remark that there is still another very interesting problem respecting the geographical distribution of our fresh-water animals, which may be solved by the further investigation of the fishes of the Tennessee River. This water course, taking the Powells, Clinch and Holston Rivers as its head waters, arises from the mountains of Virginia in latitude 37°, it then flows S. W. to latitude 34°·25, when it turns W. and N. W., and finally empties into the Ohio under the same latitude as its sources in 37°. The question now is this: Are the fishes of this water system the same throughout its extent? in which case we should infer that water communication is the chief condition of the geographical distribution of our fresh-water fishes. Or do they differ in different stations along its course? and if so, are the differences mainly controlled by the elevation of the river above the level of the sea, or determined by climatic influences corresponding to differences of latitude? We should assume that the first alternative was true if the fishes of the upper course of the river differed from those of the middle and lower course in the same manner as in the Danube, from its source to Pesth, where this stream flows nearly for its whole length under the same parallel. We would on the contrary suppose the second alternative to be well founded, if marked differences were observed between the fish of such tracks of the river as do not materially differ in their elevation above the sea, but flow under different latitudes. Now a few collections from different stations along this river, like that sent me by Dr. Newman from the vicinity of Huntsville, would settle at once this question, not for the Tennessee River alone, but for most rivers flowing under similar circumstances upon the surface of the globe. Nothing, however, short of such collections, compared closely with one another, will furnish a reliable answer. I know already from a mere catalogue of the vernacular names of the fishes from the vicinity of Jonesboro, sent me by Dr. Cunningham, and from a few specimens collected by Prof. Erni, late of Knoxville, that the fishes of the upper and lower course of the Tennessee differ greatly from

each other, without being able to tell exactly how, from want of specimens. To set this question completely at rest, it would be best to obtain collections from the different tributaries of the Tennessee, as well as from the main stream, one from the Powells, one from the Clinch, one from the Holston, one from the French Broad, &c., and from the main river, one from the vicinity of Washington, Tenn., or from Chattanooga, another from Florence, (the Muscle Shoal being the point, as I am informed by Dr. Newman, above which fish do not migrate in the Tennessee,) and another anywhere above its junction with the Ohio, perhaps best about Reynoldsburg, at some distance from the Ohio. Whoever will accomplish this survey will have made a highly valuable contribution to our knowledge.

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APPENDIX.—*Additional Notes on the Holconoti.*

HAVING lately received a large number of specimens of *Holconoti*, from California, through the kindness of my friend, T. G. Cary, Esq., of San Francisco, I avail myself of this opportunity to make several additions to my first notice of that remarkable family. As I had anticipated, the number of species belonging to it is rapidly increasing. I have now no less than six distinct species before me, presenting even a far wider range of differences than I was prepared to find among them, which has led me to establish several new genera, besides *Embiotoca*. Respecting the family characters, I have to add that there is *another space deprived of scales, extending along the middle line of the belly, from the sides of the ventrals to the base of the anal*, undoubtedly a provision to facilitate the dilatation of the abdominal cavity during the growth of the astonishingly large young of these fishes. It is rather surprising, however, that this scaleless space exists also in the males, and this might be considered an objection to the explanation just given, did we not find also tits and mammary glands in the males of Mammalia. Nevertheless *the males and females differ widely from one another*, in each of the four species of which I have thus far been able to obtain both sexes. This circumstance adds greatly to the difficulty of distinguishing and characterizing the species. The males are uniformly smaller than the females, contrary to what has been observed in the genus *Pœcilia*, in which the males (*Mollinesia*) and the females (*Pœcilia*) differ so much as to have been considered as distinct genera, but agreeing in this respect with my genus *Heterandria*, in which the males are also smaller than the females. The difference consists chiefly in the peculiar form of the anterior part of the anal in the males, which resembles somewhat that of the male of *Mallotus villosus*, being more rigid and more expanded than in the females. The jaws are more or less protractile. Air bladder large and simple. In

males the sexual aperture is at the summit of a projecting conical papilla. The genus *Embiotoca* as first established, does not require modifications; I have only to add a new species to it, and to mention some features by which it differs from the following genera: The spinous portion of the dorsal is uniformly low, so that the soft portion rises abruptly to a much greater height; the anterior *articulated* rays of the anal *simple* and *not branching* at their extremity. In the male the anterior articulated rays of the anal are swollen near their base, forming a continuous longitudinal ridge on each side of the fin. This ridge is variously modified in the different species. The jaws are moderately protractile; the lower lip is fixed by a frenum to the symphysis of the lower maxillaries, and not free and moveable all round the jaw. The young of the third new species of this genus resembles exactly those of the two formerly described, but differ remarkably from those of another species belonging to a new genus which I shall mention below, thus showing that there are generic modifications in the growth of the young, though the mode of reproduction is exactly the same in all. In Embiotoca proper, the young resemble most remarkably the mother, about the time of their escape from their confinement, except in color; in addition to the peculiarities described in my former paper, I would mention a large black diffused spot upon the anterior part of the soft portion of the dorsal and of the anal, which is found in the *young* of all three species of this genus, whilst *E. Caryi* alone shows signs of it *when full grown*. The male papilla is rather large.

*Embiotoca Caryi.*—I possess the most complete series of this species, for besides two pregnant females with young ready to escape, caught in July, I have males and females of various sizes caught in January; at this period the marsupial sac is reduced to a fusiform tube, extending from the sexual aperture to the anterior extremity of the air bladder, but the state of preservation of the intestines did not allow a minute examination of its structure. The male, which is more elongated than the female, has also much brighter colors: the longitudinal and transverse bands of the body are more distinct, the black specks upon the soft dorsal and the anal are more brilliant, and the cheeks, opercule, jaws and chin are adorned with bright blue blotches more or less confluent; the ground color of the body seems to vary from olive on the back to a yellow-orange upon the sides.

*Embiotoca Jacksoni.*—The form of the male does not differ quite as much from that of the female in this species, as in the preceding, though it is also slightly narrower. The color, as far as I can judge from alcoholic specimens, is of a deeper olive green, whilst the female is more yellowish.

*Embiotoca lateralis*, Agass.—Resembles closely E. Jacksoni in general form and appearance, but seems to bring forth its young

at an earlier period, for among several specimens caught in July, only one was full of young, and that was a younger specimen. The body is dark olive above; sides with alternate silver-gray and rusty bands; fins brown. In younger specimens the longitudinal bands are more yellow, and the fins also yellowish.

Rhacochilus, *Agass.*—In this genus the vertical fins have the same structure as in Embiotoca and the sexes differ in the same manner; but the jaws are very protractile, almost as in our southern Lachnolæmus, and the lips very fleshy, the lower lip especially broad, lobed and have their outer margin free from the jaw bone all round, and not attached by a frenum to the chin, as in Embiotoca and Amphistichus. Teeth few and only in front of the jaws, and none on the sides. The body is also more elongated. The young differ widely from those of the preceding genus: their form is more elongated, the caudal remarkably large and long and truncate at its extremity, whilst it is forked in Embiotoca; and the extremities of the dorsal and anal extend beyond the base of the caudal, whilst in Embiotoca they do not even reach it; finally there is no black speck upon either the dorsal or the anal.

*Rhacochilus toxotes*, Agass.—Color uniform olive above; sides silvery with light longitudinal bands; female darker than male; vertical fins and ventrals dark; male blackish upon opercule and cheeks. Female with mature young in July.

Amphistichus, *Agass.*—The spinous rays of the dorsal shorter than the soft rays, but gradually increasing in length, so that the soft portion of the fin does not rise abruptly higher than the spinous portion, though the anterior soft rays are the longest of the fin. Articulated rays of the anal *all divided*, and not simple in front as in Embiotoca, nevertheless the fin is separated into an anterior and a posterior portion, by the introduction in the *male* of a short flat-triangular ray, which produces a deep emargination in the outline of the fin, and in the *female* by the presence of two or three articulated rays of equal length with the others but much *stouter* and *oftener divided*. In the male the anterior rays are swollen as in Embiotoca and Rhacochilus. Papilla of the males very large. Jaws little protractile; with two rows of teeth above and below, lips thin, lower lip not free in the middle. The young have not been observed, the specimens obtained having been caught in January.

*Amphistichus argenteus*, Agass.—Bluish gray above, sides silvery with occasional indistinct and irregular transverse bands of olive color. Vertical fins yellowish.

Holconotus, *Agass.*—Dorsal long, and lowest behind, its spinous rays being the longest; the anterior and posterior parts of

this fin are not separated by a depression, but its outline descends regularly from the fourth or fifth anterior spinous rays to the posterior extremity. Structure of the anal the same as in Amphistichus but proportionally longer; the sexes differing also in the same manner. Young not known, the female obtained having been caught in January. Jaws very slightly protractile, lower jaw projecting; two rows of teeth in the upper jaw only. Lips not fleshy; lower lip free all round.

*Holconotus rhodoterus*, Agass.—Bluish gray above, silvery upon the sides with rose colored spots in irregular longitudinal lines; vertical fins, especially the caudal, reddish.

I have just been informed (February 28th) that the California Academy of Natural Sciences claims for Dr. W. P. Gibbons the discovery of the viviparous fishes upon which I had established the family *Holconoti* and the genus *Embiotoca;* but upon what ground I am not informed. This is a question in which I am entirely disinterested, having thus far been only the historian of the discovery and the biographer and godfather of the fishes. Dates and reference to other publications which may have been made in California, will easily settle the question of priority which as far as the discovery of the viviparity of these fishes is concerned, rests between Mr. Jackson and Dr. Gibbons, and not with me. I learn also, from the same quarter, that Dr. Gibbons has dedicated to me a new species of this family and that the California Academy has inscribed another species to him; but I have not yet seen descriptions of them. Should either of these species coincide with one or the other of those described above, I shall of course adopt, in the more elaborate paper, accompanied with figures, which I am now preparing upon this family, the names first established in accordance with the rules of our science.

The knowledge of this curious family is likely to lead to many other interesting disclosures. Dr. Thom. H. Webb, one of the scientific corps of the Mexican Boundary Line Commission, has sent me under date of Dec. 9th. 1853, the following abstract from his diary, dated San Diego, May 3, 1852: "Capt. Ottinger, of the U. S. Revenue service, caused his seine to be drawn for us to-day. Caught many Tiger and Shovel-nose sharks, two flounders, . . . also a number of small fish, about two or three inches long, *each of which contained ten or twelve living young*." He adds: "The viviparous progeny I exhibited to the Commissioner and several of the gentlemen of the Commission; and I also kept quite a number of them alive, in a vessel of water, for some days. In the mother they were not, so to speak, indiscriminately huddled together, but methodically arranged, and so placed in relation to each other as to form a compact series, without the loss of interstitial space, in other words, so disposed as to best accommodate the family. On leaving San Diego, I took extra pains to preserve

specimens of this fish, but these special efforts proved an injury," &c. We may therefore confidently look forward for some new type of viviparous fish from San Diego. Mr. Wm. Couper of Toronto, Canada, writes me also that an intelligent young man residing in Buffalo, New York, obtained some fish taken at Black Rock, in which a number of young were found enclosed in a pouch attached to or near the back bone, resembling the parent in form. May this not be some Cyprinodont? I am inclined to believe it, since I have of late ascertained that many of our representatives of that family, if not all, bring forth living young, though these are very small at the time of their birth.

That among our Sharks the Dogfish (*Acanthias americanus*, St.), is viviparous, has long been known. So is also *Mustelus Canis*, Mitch. But Mr. Thayer S. Abert, of the U. S. Engineers, informs me that the Stingray of the coast of North Carolina also brings forth living young. This would be, as far as I know, the first example of a viviparous species in the family of Rays.

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## Art. XXXIX.—*Observations on the Development of the "Surinam Toad" (Pipa Americana)*; by Jeffries Wyman, M.D.

(Presented to the Boston Society of Natural History.)

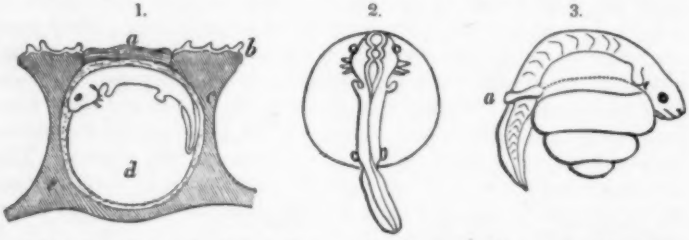
The specimens upon which the following observations were made, were obtained by Dr. Francis W. Craigin, U. S. Consul, to whom the Society has been so frequently indebted for his generous and munificent contributions to its cabinet, of zoological collections from South America. The habits of these extraordinary animals during the reproductive season are well known. The eggs are transferred by the male to the back of the female, to which they adhere, and where they are impregnated; their presence excites increased activity in the skin, it thickens, is gradually built up around each egg which it at length nearly encloses in a well-defined pouch; this process of investment has been compared by J. Müller and others to the inclusion of the mammiferous ovum by the deciduous membrane of the uterus. The opening which is left after the pouch is formed, is at length closed up by an operculum, and thus the egg is shut off from all direct communication with the air.

Of the eight specimens which I have examined, two were destitute of eggs in the back, and the skin of these presented a uniform surface throughout covered, as is usual, with conical papillæ. One of them I ascertained by dissection to be a female, the ovaries being well filled with eggs. In the backs of all of the others, ova existed in different stages of development, the number of egg-sacs varying in different specimens from forty to one hundred and

fourteen. The structure of the sac may be understood from an inspection of fig. 1, which represents a magnified vertical section through the whole thickness of the skin: *a* represents the operculum, *b* the epidermis, *c c* dermis or true skin, and *d* the yelk with its embryo. The sacs are at variable distances from each other, sometimes so closely approximated that the intervening integument is reduced to the thickness of a piece of paper. The operculum adheres to the circumference of the mouth, and there is usually found just beneath it a layer of gelatinous matter which is continuous, in some instances at least, around the whole circumference of the egg. The structure of the operculum as seen beneath the microscope was not homogeneous, but seemed to be composed of ill-defined fibres, not unlike those of the white element of areolar tissue, and there were intermixed with them granules of pigment. The interior of the pouch was covered by a layer of pavement epithelium, continuous at the orifice with the cuticle covering the surface of the body; it was easily detached, and its cells were nucleated and contained colored granules. Beneath the skin there exists over the whole back a large cavity, as in Frogs, but unlike the one in them no nerves were seen passing through it, in the region of the spine, to the integuments.

The eggs are quite remarkable when compared with those of other Batrachians for their great size, the yelk alone measuring one-fourth of an inch in diameter. In almost every instance, on removing the operculum, the embryo, however small, was found just beneath it, and thus occupying a position on the yelk which had the nearest proximity to the air.

In the earlier stages, as seen in fig. 2, the head is broad and flat, the cerebral vesicles are easily detected, the lateral portions not having united on the median line; the eyes were prominent




and black; the spinal canal was closed and the ventral laminæ were just beginning to extend upon the upper surface of the vitellus; the arms consisted of pyriform processes from anterior portion of the trunk, but the legs consisted of oval masses entirely disconnected with the parts surrounding the vertebral column, and seemed to have an independent centre of growth, and therefore did not bud out from the trunk. In all of the earlier speci-

mens three branchial appendages were visible on each side of the head. The general aspect of the embryo, as it lies extended on the surface of the yelk, reminds us of the larval condition of Salamanders and Tritons. The vitelline vessels communicated with the trunk by means of two afferrent vessels on each side of the head, and several efferrent ones on the sides of the trunk.

In a later stage as exhibited in another series of embryos (fig. 3), the external branchiæ had disappeared, the legs (*a*) now united with the trunk, were terminated by an expanded extremity, the rudiment of a foot; the ventral laminæ as represented by the dotted line extended farther down upon the yelk, but still this last was to a great extent uncovered; the nostrils were visible as round terminal depressions, but it was not ascertained if they communicated with the mouth. A small branchial fissure was detected on each side of the neck, and within this, as was shown by slitting open the mouth and œsophagus, there existed on each side a series of fringed branchial arches.

The most extraordinary feature however of this stage was the peculiar change going on in the yelk mass; the whole of the yelk substance was moulded into a spiral coil, fig. 3, and invested with a thin tunic, and thus converted at once into a spiral intestinal canal, the coils of which extend from the sides of the trunk to the most prominent portion of the yelk, and there changing direction and occupying the axis of the coil the intestine passes back again towards the trunk. The *whole* yelk-mass is therefore moulded into a spiral intestine.

4.

5.

In the most advanced stages which were examined (figs. 4 and 5) the ventral laminæ had almost entirely included the intestinal canal as seen in fig. 5; the papillæ and rows of tubercles upon the skin were developed, the intestine had increased in length, and the extremities had become elongated and were provided with well defined toes. The legs were folded against the sides of the body, usually one towards the back and the other towards the abdomen, and the tail which had become proportion-

ally much larger, was folded against the side and directed towards the head. The mouth as in the preceding instances, was not terminal, but receded a little on the under side of the head.

When the most advanced ova are compared with those which had made the least progress, as will be seen by reference to figs. 2 and 4, which are proportionally magnified representations, it is quite obvious that in the later periods, the mass of the embryo is much greater than that of the yelk and the embryo of the earlier. This increase as shown by weight was found as follows:—the embryo represented by fig. 2 weighed 2·95 grains, and that by fig. 5, 3·37 grains. It is not improbable that if earlier and later periods had been compared, the difference would have been still greater.

In none of the instances which fell under my notice had the final metamorphosis taken place. But Bonnet, Dumeril and others have observed that the tadpole remains in the dermal sac until its limbs are perfectly formed and the tail has been absorbed, until in truth it arrives at the same stage reached by the common toad, when having finished its aquatic life to which it is no longer adapted, it leaves the water and seeks its livelihood in a more congenial manner on the land.

*Remarks.*—In addition to the unusual circumstances under which these animals are developed, it will be seen that they are objects of especial interest in connection with the general subject of the development of Batrachian reptiles. The first peculiarity which may be noticed is as to the period at which the arms and legs make their appearance. The tadpoles of frogs and toads acquire a comparatively advanced stage of development before any traces whatever of limbs are seen; they leave the egg, increase in size, and after a period (variable in the different species) of weeks or months, the rudimentary arms and legs begin to be formed. In Pipa they appear before the external branchiæ disappear, and before even the ventral laminæ descend upon the sides of the vitellus. The development of the limbs, independently of the vertebral column, is an interesting morphological fact.

That the first trace of a leg is an oval mass, entirely free and detached from the vertebral axis, goes to show that whatever view we may adopt, with regard to the homology of legs, the pelvis included, they are something superadded to it and not evolved from it, or any of its processes. I have ascertained by direct observation, that even among Frogs, the legs which appear on each side of the tail, in the form of small papillæ, are primarily tegumentary growths, that beneath these there is developed a cartilaginous plate which gradually extends upwards on each side until it meets with the transverse processes of the vertebral column, with which it becomes permanently connected under the form of the pelvis, and at the same time the papillæ are de-

veloped into limbs with their contained bones; thus the pelvis which in the adult seems to be an appendage to the vertebral column, is in the embryo an independent structure, just as the tooth is primarily independent of the jaw. In this mode of the development of the legs, we have a temporary analogy to the permanent constitution of the same parts in Fishes, in which the ventral fins are never connected with the vertebral column by their pelvic bones, these being confined to the abdominal surface of the body.

The complete development of the tail, adapted to swimming, is under the circumstances worthy of attention. In the ordinary Ranidæ the phases of development are in accordance with the peculiar conditions under which the earlier periods of life are passed; their habits are not only wholly aquatic, but they have many of the anatomical and physiological characters of fishes, among which may be mentioned the existence of branchiæ, for aquatic respiration, and a broad and compressed tail for aquatic locomotion. The embryos of Pipæ differ from those of other allied genera, in passing through all of their embryonic phases in closed dermal sacks, where they neither breathe by the action of aquatic currents, nor are capable of executing the ordinary locomotive movements; yet the external branchiæ are developed, disappear and are replaced by internal branchiæ, and these in turn by lungs; the tail also acquires its full development with swimming adaptations, in the form of muscles and folds of skin, as in other tadpoles, and after having existed for a certain period, is removed by absorption, without having been even once made use of as a locomotive organ. It appears that in this particular instance the exigencies of embryonic life do not require the existence of a tail for the purposes of locomotion, and its presence seems to be accounted for only on the supposition of the existence of a preestablished plan according to which Batrachians generally are developed, and this plan is adhered to, although the organ may not be used or not used in the same way as in the other species. It is possible that the materials of the tail serve as a store of nutritive substance, though this seems scarcely probable; but even if this be the case, it is none the less a fact that the part assumes a structure, the adaptations of which have reference to a function wholly different.

As regards the existence of branchiæ, I have observed an analogous instance in the embryos of *Salamandra erythronota*, where these organs are developed externally, though the eggs are deposited under a log, and the animal is not aquatic at any period of life.

The only other subject to which it is proposed to refer, is the growth of the embryo, by which there is formed at the end of incubation a larger mass than existed in the egg when it commenced.

This increase in bulk could have been effected in no other way than by an absorption of materials furnished by the dermal sac, since the existence of an operculum would prevent the entrance of nutritive matter from without. The gelatinous matter which originally surrounded the egg, may have contributed something, but still there is growth after this has disappeared. It seems highly probable, therefore, that the walls of the pouch secrete the necessary additional quantity to supply all that is required for development. In so far as observation has extended, this is a solitary instance among Batrachians if not among Reptiles generally in which the embryo is nourished at the expense of materials derived from the parent.

Among fishes analogous facts exist. Dr. John Davy has shown that the undeveloped but mature egg of the Torpedo in June does not exceed 200 grs. in weight, whereas in the month of September the fœtuses weigh from 428 to 580 grs. The recently discovered genus, *Embiotoca*, Agass., exhibits to us not only the fact of a true ovarian gestation as in Pœcilia, but that the young continue to grow in the ovary till they acquire a very extraordinary size. In *Embiotoca Jacksoni*, for example, Professor Agassiz states that a specimen 10½ inches long and 4½ inches high, the young were nearly three inches long and one inch high. He was led to the belief that in this case the water entered the ovarian sac, since the gills were found so highly developed. If this supposition should prove correct, it still seems quite improbable that the young fish should acquire all the materials for its growth from whatever may be in this manner casually introduced.

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ART. XL.—*Researches on the Development and intimate Structure of the Renal Organs of the four Classes of the Vertebrata;* by W. I. BURNETT, M.D., Boston.

THERE are two facts strikingly indicative of the importance of the urinary organs in all the higher forms of animal life. These are: first, their widely distributed presence; and second, their early appearance in developing embryonic forms; I might, perhaps, add as a third fact, that their functional activity is usually in pretty exact ratio with the grade of organization.

Throughout the higher classes of the Invertebrata,* and in all the Vertebrata, these organs are present, and their functional relations quite prominent; it is only in the lower classes of the animal kingdom, where there is an absence or incompleteness of a

* Urinary organs are not found lower than the Cephalophora, in which, as also in the Cephalopoda, Crustacea, Arachnoidæ, and Insecta, they assume an important function, and are often of a complex structure. See Comparative Anatomy, by *Siebold* and *Stannius*, transl. &c. by *Burnett*, vol. i, §§ 223, 255, 288, 314, 345.

true circulatory system distinct by itself, that these structures are wanting. The kidneys are organs inseparably connected with an important blood-function, and in the Vertebrata where the conditions of organization rest upon an active and widely distributed circulation, they possess determinate anatomical and physiological characteristics, and may therefore form the subject of a distinct and complete investigation.

On this account, I limit myself in the present paper to an examination of these organs as observed in the four grand classes of Vertebrata: Fishes, Reptiles, Birds, and Mammals,—and by the terms *Urinary Organs*, I mean those both of a transient and a permanent nature—the Wolffian bodies of most embryos, as well as the true kidneys of all adults.

I have lately enjoyed excellent opportunities for the study of the development and intimate structure of these glandular organs; and their apparent peculiarities, as observed in the four different classes, require the detail I have given, in order to convey a clear idea of what may be called the *formula* of their organization. I say formula, for the intimate organic structure of a urinary organ wherever observed among the Vertebrata, is invariably the same; the variations being only apparent and extrinsic, and due to the many modes of combination of a single individual structure.

As before mentioned, the Renal organs appear under two forms, viz.: the Embryonic, or the so-called Wolffian Bodies, and the Permanent or true Kidneys.

## I. *Wolffian Bodies.*

To the physiologist it is a beautiful and suggestive fact, not unfrequently observed in embryological studies, that nature sometimes puts up a temporary, provisional structure, for the performance of an important function, until the conditions of the general organization shall be so far advanced that there can be formed a permanent organ of a certain type, belonging to the animal as such, and which persists through its entire life. Such a fact is presented by the Wolffian bodies.

In the higher forms of organization, the blood, directly upon its active circulation, seems to require some means for the removal from it of certain effete particles, and to effect this a delicate, transient structure is erected, to remain only until a permanent one in the shape of a true kidney can be formed.

These temporary kidneys are found in the embryos of all the Vertebrata, excepting the Fishes and the Amphibian Reptiles; that is, in the true Reptiles, the Birds, and the Mammals. The length of time they persist is, in general, in an inverse ratio to the grade of the animal; in fact, this law of gradation seems so marked, that there would at first seem some ground for the opinion that in the Amphibia and the Fishes, which have only per-

manent renal organs, these last may be only persistent Wolffian bodies, but this point will receive our attention at a future time.

Wherever occurring, these Wolffian bodies present the same unvarying form and type of structure; their mode of development I have found to be equally invariable, whether occurring in Reptiles, Birds, or Mammals. They always make their appearance during the very earliest phases of development of the embryo, and, with the exception of the heart, are the first organs formed in the abdominal cavity.

As I have studied these phases of development in Birds more carefully than in the other classes, I will describe them as observed in the chick; and this description will include what belongs to these organs in Reptiles and Mammalia, in all their essential details.

In the Chick, there appears, at about the fiftieth (50th) hour of incubation, a line on each side of, and lying close to, the vertebral column; this line extends from near the region of the heart to the caudal vertebræ, and is composed of a collection of nucleated cells which soon become arranged so as to form a tube; this tube is the basis of the future Wolffian body. At this stage of development there is observed, then, a simple tube on each side of the vertebral column; but a few hours after, the surface of the tube becomes nodulated at regular intervals on its inner surface. These nodulations are the beginnings of a series of eversions from the main tube which soon therefore has a digitated appearance—each of the finger-like prolongations being a future uriniferous tube. These prolongations having been formed from without inwards, the original tube, which now has become a common duct, lies upon the external side, and the former overlap the vertebral column.

This formation of tubes by deverticula from a main one, constitutes the first phase in the development of this organ as a compound structure. The second phase is the formation of a direct connection between the blood-vessels and these newly-formed tubes, so that an eliminating function may be performed. This occurs at about the sixtieth (60th) or sixty-fifth (65th) hour of incubation. At this time, the free extremities of some, but not all, of the newly-formed tubes become enlarged and dilated into an infundibuliform body which, together with its attached tube, resembles a flask with a very long neck. *In* this dilated extremity of the tube, a knot of blood-vessels is formed anew from epithelial cells; this occurs in a manner so beautiful that I digress here for its more careful description.

This infundibuliform or bulged end of the tube is nothing but the tube at this point taking on a little larger growth; it is therefore lined, like the tube itself, with a layer of epithelial cells. These cells, by a more or less linear arrangement, form a minute

convoluted tube enclosing in its calibre smaller cells; this tube is the future blood-vessel which connects afterwards with the blood-vessels of the general circulation, and then the enclosed smaller epithelial cells become blood-corpuscles. This convoluted blood-vessel is the so-called Malpighian tuft, or the Glomerulus, and is the functional or active structure of the organ.

A brief but comprehensive description of the structure of the Wolffian body would then be: a straight, main duct into which empty many digitiform tubes, the capsular dilatations of the free ends of which contain each a knot of blood-vessels. In the same way, I might describe the method of its function, as: the straining off from the blood, through this knot of vessels, those effete particles which as a whole form the urinary excretion of the embryo. A structure more simple cannot easily be conceived of, yet its function is most effectual, for although thus quickly formed, we shall soon learn that both the structure and function of the complex permanent kidney rest upon precisely the same primitive types.

These temporary kidneys, thus formed, which have, as before remarked, exactly the same structure wherever found, have a duration varying in the different classes, which stands in an inverse ratio to the grade of the animal.

In the true Reptiles, and in Birds, they persist as active functional organs during a considerable portion of the embryonic life; in fact, their disappearance coincides with the appearance of the kidney as an active structure. Their structural remains, however, are often found long after the animal has passed out of its dependent, vitelline life; thus, in the chick the kidneys assume the urinary excretion at about the tenth (10th) day, yet the vestiges of those bodies may often be observed after hatching; and in the Alligator, I have seen, even four or five months after the animal had escaped the egg, the remains of these organs so well preserved that the Malpighian bodies were distinct in them. But in the Mammalia, where their existence as active parts is very brief, in fact so limited that it is difficult to observe them in a state of functional activity, they are correspondingly soon absorbed, and although these remains are observed more or less distinctly after birth in some species, yet, generally, they have mostly passed away by the latter half of the intra-uterine life.*

* *Müller*, (Physiologie, transl. by Jourdan, &c., Deux. éd. Paris, 1851, ii. p. 760, fig. 290, C. 5,) has given a figure representing the remains of the Wolffian body in a human fœtus of 3½ inches. As is well known, there may be observed during the last months of the human fœtus, or even after birth, peculiar canaliculi situated in the fold of the peritoneum and of the Fallopian tube. They form the so-called organ of *Rosenmüller* (De ovariis embryonum, Leipzig, 1801), and are probably the remains of the Wolffian body. In the Ruminantia, Solipeda, and Suina, there are likewise canaliculi (the canals of *Gœrtner*), with thin walls, which extend from the large ligament of the uterus along the neck of this organ, pass between the mucous

As I have shown elsewhere,* the receptacle of this urinary excretion of the Wolffian bodies is the Allantois, which, as I have described in the paper referred to, is at first, properly nothing but their receptacular appendix—the excretory ducts terminating in it below.

In conclusion, I may remark that the Wolffian bodies are transient in all their relations; they subserve nothing for the formation of the permanent organs that succeed them—being every way distinct structures; and the testicles or ovaries which are first observed on their bodies, have no other relation with the part on which they rest except that of mere contact.

But before leaving this section of the subject, I think it necessary to explain one point which otherwise might seem assumed on too little authority. I refer to the statement I have already made that the Wolffian Bodies are absent in the Amphibian Reptiles. This, as is well known to physiologists, is directly opposed to the views of Müller, and therefore demands here a special reference.

Among Müller's earliest anatomical publications, upon the formation of glandular structures and particularly those of the genital apparatus, he pointed out and gave a careful description† of what he regarded as a new structure in the embryos of Amphibia. Up to this time, the so-called Wolffian bodies had not been observed by Meckel, and Rathke, who had specially studied them, in either the Fishes or the Amphibian Reptiles,—and these newly-discovered structures Müller regarded as a peculiar form of the Wolffian bodies. He described them as two organs situated, one on each side of the vertebral column, directly under the branchiæ of the larvæ of Batrachians, and from which proceeds a duct that runs along the side of the vertebral column and opens, finally, into the lower portion of the intestinal canal.

But the presence of these organs, thus carefully described and especially by so excellent an authority, has not been acceded to by all, although by most, subsequent microscopical anatomists;‡

membrane and the muscular tunic of the vagina, and open, at last, near the urethral orifice. These, *Jacobson* (Die Oken'schen Körper oder die primordial Nieren, Kopenhagen, 1830, p. 17) thinks are the remains of these same bodies; a view regarded as probable likewise by *Rathke*, *Valentin*, and *Gurlt*, but which certainly requires new proofs. See, in this connection, *Rathke*, in *Meckel's* Arch., 1832, p. 386; *Valentin*, Handbuch der Entwickelungsgeschichte, p. 393; *Gurlt*, Vergleich. Anat. &c., ii, p. 115; and *Bischoff*, Entwickelungsgeschichte, &c., *Jourdan's* edit., p. 348.

* See a paper, *On the formation and function of the Allantois*, in the Proceed. Amer. Acad. of Arts and Sciences, Boston, for October, 1852.

† *J. Müller*, Bildungsgeschichte der Genitalien, Dusseldorff, 1830. See also his De Glandularum secernentium structura penitiori earumque prima formatione, 1830, p. 86, Tab. xii, and his Physiologie, transl. by *Jourdan*, &c., Deux. éd. Paris, 1851, ii, p. 758.

‡ Among those who have followed *Müller*, may be mentioned *H. Meckel*, (Zur Morphologie die Harn—und Geschlechtswerkzeuge der Wirbelthiere, Halle, 1848,) and *Reichert*, (Das Entwickelungsleben im Wirbelthier-Reich, 1840, p. 26).

at least that they are distinct organs as independent of the permanent kidneys or as the ordinary Wolffian bodies of Birds and Mammals.

This is a point to which I gave special attention in making these investigations, and after the most careful and repeated examinations of the larvæ of Batrachia, in all their stages, I have wholly failed to find a structure, such as Müller has described, distinct from the developing kidney, and which would correspond to the ordinary Wolffian body. On the other hand, all that I have observed in this respect is, that the common duct (future ureter) of the forming kidney extends quite high up towards the cardiac region, some distance beyond the upper limit of its branching out into uriniferous canals. The upper end of this free portion of the duct is convoluted and seems to have some direct connection with the blood-vessels, though I have never here observed any thing like Malpighian bodies. As the development of the kidney progresses, this free extremity of the tube gradually disappears, and towards the end of the larval state is observed only in remains. In brief, then, I have observed no distinct, temporary urinary organs in the undeveloped forms of Amphibia.

These observations, made for the most part during the summer of 1852, though repeated from time to time until quite recently, I was pleased to perceive confirmed by so excellent an observer as Wittich in a paper published in the autumn of the same year.* Wittich's investigations have been very extended, and as far at least as they relate to the renal organs, furnish results much like my own. He found nothing in the larvæ of the naked Amphibia which would correspond to Müller's Wolffian bodies, excepting the free prolongation upwards of the main duct or the ureter of the developing kidney.

The doctrine here insisted upon that the true Wolffian bodies, or fœtal kidneys, are present only in the embryos of the true Reptiles, the Birds and the Mammals, is so apposite with the results I have obtained from an investigation of the Allantois, that it may be urged with an added force. In the paper already alluded to, I have sought to show that the Allantois is, primitively, the vesicular expansion of the combined extremities of the ducts of the Wolffian bodies, and finally becomes the receptacular appendage of these organs—its contained liquid being properly urine.† But afterwards it subserves also another function—that of the aeration of the blood.

* *Wittich*, Beiträge zur morphologischen und histologischen Entwickelung der Harn—und Geschlechtswerkzeuge der nackten Amphibien, in *Siebold* and *Kölliker's* Zeitsch, f. wissensch. Zool., iv, 1852, p. 125; also, Harn- und Geschlechtsorgane von Discoglossus pictus und einiger anderer aussereuropäischer Batrachier, ibid, p. 168.

† *Jacobson*, as is well known, detected urea in the liquid contents of the Allantois of very young embryos. See loc. cit.

This view being correct, we could expect to find Wolffian bodies only where there is observed an Allantois, and *vice versa*. Thus, in the Fishes and Amphibian Reptiles, which truly have no Allantois, there would be no temporary kidneys or the Wolffian bodies; while in the other Vertebrata, which are allantoïdian, these last would be found. I scarcely need refer further to this parallelism as far as regards our doctrine in question.*

## II. *Permanent or true Kidneys.*

These organs being apparently indispensable to the adult economy of all the Vertebrata, have a physiological importance of the highest character. Although the primitive essential type of their structure is precisely like that of their temporary analogues just described, when present, yet as they are permanent organs and sustain definite relations and connections with various parts of the general system, the study of their structure as organs is the more complex and interesting. It would be foreign to my subject to enter upon the details of the comparative Anatomy of these organs, describing those variations of external form belonging to each type;† I shall limit myself to those points that bear directly upon the subject of development and intimate structure.

As a leading fact I may mention that the idea or *formula* of the kidney, wherever observed in the vertebrate, is always the same; and thus finding the intimate structure essentially alike everywhere, we should naturally infer that there is throughout a single and invariable mode of development. This indeed, I have found to be the case, as I hope to show in the following details.

In the Fishes and Amphibian Reptiles, where there are no temporary urinary organs, the earlier phases of development of the kidneys seem to be a little less complicated than in the higher classes. In fact, the type of structural development here is quite allied to that of the Wolffian bodies, and by some anatomists

* I much regret that in preparing this account of the Wolffian bodies, I should not have had direct access to the work of *Follin*, (Recherches sur les corps de Wolff, Paris, 1850), which I have failed to get on the reputed ground of its being out of print. Considerable reference, however, is made to this work, by the copying of some of its figures, in *Littrê's* edition of *Jourdan's* translation of *Müller's* Physiologie, Paris, 1851, ii, p. 761. But by the figures in question, I can judge nothing of the views of *M. Follin* upon this structure.

Besides the well known works of *Rathke*, *Meckel*, *Baer*, *Jacobson*, *Valentin*, *Bischoff*, already referred to, see especially for representations of these bodies, *Müller*, Bildungsgeschichte der Genitalien; his De Glandularum secernent. &c.; and his Physiologie, Transl. by *Jourdan*, Paris, 1851, ii, p. 757. For figures of the so-called Wolffian bodies of Amphibia, see *Wittich*, loc. cit., Taf. ix, figs. 1, 2, 3, 4, 5, 6, &c.

The general structure of these bodies has long been known, and I am not aware that I have added any new facts in this respect. My observations relate chiefly to the very first stages of formation of all the parts, and concerning this, have furnished results I have seen recorded by no investigator.

† For full details on this subject, with a copious reference to its literature, see the Comparative Anatomy of *Siebold* and *Stannius*, transl. &c. by *Burnett*, ii, §§ 49, 106, 153, 260.

these organs in Fishes have been regarded as merely permanent forms of the above mentioned bodies. This last, however, I do not think correct, for in this class the subsequent phases of development by which the organ is increased in size, are exactly like those of the general formation of these organs in Birds and Mammals.

The formula of development is the branching out indefinitely of a primitive tube which is the future ureter;—the terminal portions of these branches forming an intimate connection with the general vascular system, either by Malpighian bodies, or by a delicate net-work of vessels.

The mode of formation is therefore arborescent, and the analogy of the development and growth of this gland with that of vegetable forms is too striking not to be noticed. We shall see that the growth exactly resembles that of a tree even almost in its details.

The line of development being everywhere the same, I will, for the sake of uniformity, describe it as occurring in the Chick, where, moreover, I have carefully registered the successive phases.*

In the chick no traces of the kidney are perceptible, according to my own observations, until the end of the fourth or the beginning of the fifth day. The ureter, then, is the part first seen, and consists of a simple tube, the upper part of which sends off branches. Each of these branches then divides and subdivides,

* Here I may perhaps well allude to the views of other observers who have specially studied the development of the kidney. The authors best known in this respect are *Rathke*, *Müller*, *Valentin*, *Bischoff*, as well also as *Wittich* of later times. The works of these men have already been cited. According to *Rathke*, the first traces of the kidney consist of a number of small claviform protuberances appearing in an amorphous blastema situated in the place of the future kidney. These he says are pediculated, and these pedicles ultimately become attached to the ureter, which at this time is not yet formed. In these claviform bodies the uriniferous tubes subsequently appear, but in a manner not precisely determined.

*Valentin's* investigations agree in general with those of *Rathke*, but, from some observations made upon very small embryos of the hog, he concluded that the ureter, pelvis, and uriniferous tubes were primitively developed separately, being subsequently united. *Valentin's* view of the formation of the uriniferous tubes coincides with that of *Henle*, viz.: that it occurs by the condensation of a linear arrangement of the vesicular contents of the blastema.

Both *Müller* and *Bischoff* confirm, in general, the views of *Rathke;* but *Bischoff* differs not a little upon some points which deserve mention. He says, "I have never been able to convince myself that the ureter, pelvis, and canaliculi are developed separately, and I think that their rudiments are continuous throughout. They are not hollow at first, and their internal cavity is the result only of further development."

But of all, *Wittich's* results most closely approach my own. Speaking of these organs in the Amphibia, he says: "The further growth of the kidney occurs, it would appear, partly by a new deverticulation of the excretory duct (ureter), and partly by a widening, lengthening and branching of the primitive deverticula."

As will be seen in the text, the view I advance is the arborescent mode of development, more or less marked according to the class, of these organs—the formula being a more or less extended deverticulation of tubes from a main tube or duct.

and around these subdivisions are collected a mass of blastematous cells. These cells serve as the material for the growth and further division of the tubes—in fact for their ultimate ramification, until the whole structure is completed. There is observed, therefore, at an early period, a main stem (ureter) which has many branches, each of which is the foot-stalk of a leaf-like body; this last is one of the future lobules of the kidney. The whole plan of structure is here laid out—a branching ureter with lobules. The increase of the size of these lobules, and the formation of the ultimate uriniferous tubes takes place by one and the same process, viz.: by the branching of the original tubes in a plumate form from a main tube,—the whole resembling the plume of a feather. The tubes do not end, however, on the edge of the plume (so to speak), but here loop and return, and when near the shaft or main tube, they dilate into Malpighian bodies; this is, as far as I have observed, their invariable mode of termination, there being no anastomoses of the tubes as some have supposed.

The mode of formation of the Malpighian body of the true kidney, is precisely like that of the same structure belonging to the Wolffian body, already described; I need not therefore here repeat the description. I may remark, however, that the two blood vessels which compose the glomerulus, usually penetrate the capsular dilation of the tube at some point more or less near the opposite one of its insertion upon the tube, and rarely more laterally, or at least on the inner half of the capsule. In the chick, the Malpighian bodies begin to appear at about the tenth day; they are then very few in number, but they become more numerous exactly in proportion to the growth of the organ—not ceasing to be formed until the kidney has reached its full size.

Nothing could be simpler therefore than the mode of formation of the renal structure; it is clear and unmistakable—one tube gives rise to another by an eversion of its walls, and this last produces another in the same way, and so on.

Such is the mode of development as occurring in Birds, and my observations upon it as found in all the other classes, show that the formula is there invariably the same. There are, however, peculiarities of the combination of this formula in each class, which demand a special consideration.

In Fishes, these organs appear at an early period as two straight tubes, extending, one on each side of the vertebral column, from the region of the heart to the anus. These tubes soon give off from their inferior and inner surface, *deverticula*, exactly as with the Wolffian bodies just described, and for a time the organs have much the same appearance as embryonic kidneys. But afterwards, as the animal increases in size, a new development takes place. This consists in the branching, dichotomously, of the deverticular tubes,—a process which goes on indefinitely, giving

the whole organ much the same lobulated, arborescent structure as that of the chick—although the plumate arrangement of the terminal tubes is not here present. In both the Cartilaginous and the Osseous Fishes, the functional or secreting structure is, as far as I have observed, always the same; that is, the tubes invariably terminate each in a well-formed Malpighian body.

In the Amphibian Reptiles, the kidneys quite closely resemble those of Fishes, yet simulating even more closely than these, the general character of the Wolffian bodies. They present little tendency to an aborescent structure, the tubes of the main duct rarely dividing but rather forming convolutions, and ending invariably, according to my own observation, in Malpighian bodies.* These Malpighian bodies, together with their adjacent portion of the uriniferous tube, are lined with long lash-like cilia, the ever constant, rapid action of which, waving towards the outlet, presents a beautiful aspect. The use of these cilia is, evidently, to direct the course of the current out of the Malpighian body, thereby keeping this last in a free, unobstructed state. In the true Reptiles, the development of the kidney is arborescent as in Birds, and in the Serpents the lobules thus formed remain more or less separate through life. This is also the case, but in a less degree, in the Chelonioidæ.

But the Ophidioidæ present this peculiarity, that all the uriniferous tubes do not end with a Malpighian body; in fact, I have been able to find but few of these bodies which are usually situated on the posterior portion of the organ. Many of the uriniferous tubes terminate in a simple cæcal extremity, and here the ultimate connection with the vascular system by means of a Malpighian body, is replaced by a large system of renal vessels, the indefinite ramifications of which permeate the whole organ and by very delicate anastomoses bring every such cæcal secreting tube within the influence of a blood vessel.†

In the Chelonioidæ, the kidneys quite closely resemble, in every point of view, those of Birds. The uriniferous tubes have the same form of distribution and terminate always, each, in a Malpighian body.

As for these organs in the Birds, I have described their peculiarities in speaking of the chick, and have only to add that throughout the entire class there seems but little or no variation.

In Mammalia, the course of development followed is like that observed in Birds, but the subsequent changes by which the organ becomes more compact, often entirely conceal the original forms. By observations upon very young embryos in the different fami-

* For representations of the intimate structure of the kidneys of Amphibia, see, especially, *Wittich*, loc. cit. Taf. ix.

† There is, as I have lately observed, a similar relation between the blood-vessels and the terminal cæca of the poison-gland of the Rattle-snake (*Crotalus*—a kind of *Rete mirabile.*

lies, it is evident that the development is arborescent, forming lobules,—and these lobules are at first exactly like those observed in Serpents and Birds. In some species, as is well known, these lobules remain distinct through life; this is well seen in the otter, bear, and whale, and in many of the Ruminantia, these remains are distinctly visible. It is only in the higher forms, that they have so coalesced as to be concealed.

This intimate combination of parts produces the greatest amount of secreting surface in the smallest space. Take, for instance, the kidney of Man; here the lobules are arranged in a half-circle around a common cavity or the pelvis. But they are so united as to become conical in shape (with the bases of the cones at the surface of the organ) thereby producing the so-called pyramids. These pyramids are composed in part of tubes that spread out in a fan-like manner from a common point (calyx), and the gradually increased size of the medullary portion is produced by the branching of the tubes in a dichotomous manner. The straight regular way in which these tubes run, seems to be due, in a measure at least, to the mechanical pressure to which they are subjected by the combination of the lobules,—for I have been unable to perceive it in very young embryos, and, moreover, it does not agree with the invariable arborescent conditions of early formation. The so-called Medullary portion of the kidney is made up of fasciculi of straight tubes which divide dichotomously, but have no Malpighian bodies; they continue directly to the Cortical portion which is composed of more or less convoluted tubes—the result of the dichotomous division. These tubes finally end, each, in a Malpighian body. These Malpighian bodies sometimes lie upon the surface, directly beneath the capsule, but often also the tubes run up to the surface, loop, return a short distance, and end in a Malpighian body deeper in the renal structure. From all I have observed, it appears that in Man the Malpighian body is the *only* termination of the final tubes, there being therefore as many of the former as of the latter. I have seen nothing like an anastomosis of the final tubes, as some have supposed is sometimes the case. The pelvis of the human kidney, as also that of other mammals, is formed by the dilatation of the main duct, or ureter, involving the primary branches which give off the straight tubes of the medullary substance. In this way, and by the union of the bases of the pyramids, the apices are left free, projecting into this pelvis or main cavity. These changes I have enjoyed an opportunity to trace in an embryo.

It will be seen, therefore, that the development of the kidney in Man and the higher mammals involves no new phases,—the differences of general structure being extrinsic and due to combinations which produce compactness and decreased size without a corresponding decrease of functional surface.

In the foregoing account, I have preferred not to burden the general text with a reference to the history and criticism of some of the most important as well as disputed points connected with this subject. These can better be discussed by themselves.

The most important of these points is the character, relations, and connections of the Malpighian body. This secreting organ of the kidney had long been recognized by both the older and the more modern observers, but its intimate structure and its connections with the other parts of the renal substance as the functional secreting organ, were first successfully made out and published by Bowman* in a memoir which has since become classic on this subject. I may here mention that it is quite a remarkable historical fact that more than fifty years before, Schumlasky† expressed the view that these bodies were the source of the urinary secretion and had direct connection with the uriniferous tubes. But this view was opposed by subsequent observers. In 1841, Müller‡ published a portion of his work on the anatomy of the Myxinoid Fishes, where he describes the kidney as consisting of sac-like deverticula from a main tube or ureter, terminating cæcally, and in the end of which is a small tuft or knot of blood-vessels. The value of this discovery was not appreciated until Bowman (ignorant, however, at this time of Müller's investigations on this point) published the next year his memoir. To Bowman, therefore, we seem indebted for the first full exposition of the secreting structure of the kidneys of Vertebrata.

Bowman's doctrine was, that the Malpighian body is the infundibuliform expansion of the uriniferous tube, and that the glomerulus or tuft of blood-vessels lies enclosed freely in this expansion, being composed of a tortuous loop, the two component vessels entering at the same point which is generally opposite or nearly so to the point of insertion of the uriniferous tube. The Malpighian body thus composed, Bowman maintained, is the exclusively secreting structure of the kidney. These are the essential features of the results advanced, and I need not enter into the details of a memoir so well known as this.

Results so valuable in physiology, were, of couse, examined by different investigators on every side. Reichert,§ in his report upon the progress of Microscpical Anatomy for 1842, enters into a critical discussion of this subject. He denies that the uriniferous tubes end in the capsule, and regards this last as a distinct and separate formation. He also denies the presence of ciliated epithelium in the Malpighian body, and the uriniferous tubes.

* *Bowman.* Philosophical Transactions, London, 1842, Pt. i, p. 57.
† *Schumlasky.* De structura renum. Strasburg, 1788.
‡ *Müller.* Vergleich. Anat. d. Myxinoiden, Dritte Fortsetzung. Berlin, 1841. p. 13.
§ *Reichert,* Bericht über die Fortschritte der mikroskopischen Anatomie in dem Jahre, 1842, in *Müller's* Arch., 1843, p. ccxvii.

On the other hand, he maintains that the glomerulus or knot of blood-vessels is enclosed in the Malpighian body. These views he has defended in some of his subsequent reports.*

In 1845, Gerlach† published a still different opinion upon this disputed point of the Malpighian body. He maintained that the uriniferous tubes do not end in flask-shaped cæca, but loop and pass into each other. The Malpighian bodies he declares to be globular deverticula from the tubes, and that they contain, as Bowman advocates, the glomerulus. This author has likewise reasserted his views in a subsequent paper.‡

The view of Bidder§ is even still different. According to this observer, the uriniferous tubes end in flask-shaped cæca, but these last do not enclose the glomeruli. On the contrary, the Malpighian bodies receive each a glomerulus in a kind of depression, and so the cavity of the Malpighian body is as disconnected from the glomerulus as is the cavity of the pleura from the lung.

These, as far as I am aware, are all of the dissimilar views maintained by recent observers. It is indeed remarkable that there should have occurred such a discrepancy. Most other investigators‖ of note, follow more or less completely the views of Bowman. Among these may be mentioned Kölliker,¶ Patruban,** Nicolucci,†† Mandl,‡‡ Victor Carus,§§ and Wittich.‖‖

As to my own views, they will appear sufficiently plain in the foregoing pages. I subscribe to Bowman's results very generally, as to the relations of the Malpighian body to the tubes and to the glomerulus. In tracing the successive phases of the development of these parts, this point enlisted my special attention from its disputed character. The Wolffian bodies are the best structures for study in reference to this point, and, having traced in these the budding out, as it were, of the uriniferous tubes,—their dilatation, at the extremity, into a flask-shaped body, and the formation, in this last, of the Malpighian tuft or glomerulus.—I can have no doubt that all these connections and relations really exist.

* *Reichert*, see Bericht, &c. for 1848, in *Müller's* Arch., 1849, p. 65; also that for 1849 in ibid, 1850, p. 67.

† *Gerlach*, Beiträge zur Structurlehre der Niere, in *Müller's* Arch., 1845, p. 378.

‡ *Gerlach*, Zur Anatomie der Niere, in *Müller's* Arch., 1848, p. 103.

§ *Bidder*, Ueber die Malpighischen Körper der Niere, in *Müller's* Arch., 1845, p. 508.

‖ *Hyrtl*, however, does not follow *Bowman*, but rather *Gerlach*, that is, that the uriniferous tube does not end in the Malpighian body. See Beiträge zur Physiologie der Harnsekretion, in the Zeitschrift der Gesellschaft der Aerzte zu Wien, ii. p. 381.

¶ *Kölliker*, Ueber Flimmerbegungen in den Primordialnieren, in *Müller's* Arch. 1845. p. 518.

** *Patruban*, Beiträge zur Anatomie der menschlechen Niere, in the Prager Vierteljahrsschrift, xv, p. 87.

†† *Nicolucci*, Sull' intima struttura dei reni in Filiatre-Sebezio. Feb., 1847.

‡‡ *Mandl*, Mémoire sur la structure intime des Organes urinaires, p. 18.

§§ *Victor Carus*, Ueber die Malpighi'schen Körper der Niere, in *Siebold* and *Kölliker's* Zeitsch. f. wissenschaftl. Zool., ii, 1850, p. 58.

‖‖ *Wittich*, Beiträge zur Anatomie der gesunden und kranken Niere, in *Virchow* and *Reinhard's* Arch., iii, p. 147.

ART. XLI.—*The Numerical Relation between the Atomic Weights, with some Thoughts on the Classification of the Chemical Elements;* by JOSIAH P. COOKE, Jr., A.M., Erving Professor of Chemistry in Harvard University.*

NUMERICAL relations between the atomic weights of the chemical elements have been very frequently noticed by chemists. One of the fullest expositions of these relations was that given by M. Dumas of Paris, before the British Association for the Advancement of Science, at the meeting of 1851. This distinguished chemist at that time pointed out the fact, that many of the elements might be grouped in triads, in which the atomic weight of one was the arithmetical mean of those of the other two. Thus the atomic weight of bromine is the mean between those of chlorine and iodine; that of selenium is the mean between those of sulphur and tellurium, and that of sodium, the mean between those of lithium and potassium. M. Dumas also spoke of the remarkable analogies between the properties of the members of these triads, comparing them with similar analogies observed in organic chemistry, and drew, as is well known, from these facts arguments to support the hypothesis of the compound nature of many of the now received elements. Similar views to those of Dumas have been advanced by other chemists.

The doctrine of triads is, however, as I hope to be able to show in the present memoir, a partial view of this subject, since these triads are only parts of series similar in all respects to the series of homologues of organic chemistry, in which the differences between the atomic weights of the members is a multiple of some whole number. All the elements may be classified into six series, in each of which this number is different, and may be said to characterize its series. In the first it is nine, in the second eight, in the third six, in the fourth five, in the fifth four, and in the last three. The discovery of this simple numerical relation, which includes all others that have ever been noticed, was the result of a classification of the chemical elements made for the purpose of exhibiting their analogies in the lecture-room. A short notice of this classification will, therefore, make a natural introduction to the subject.

Every teacher of chemistry must have felt the want of some system of classification like those which so greatly facilitate the acquisition of the natural-history sciences. In most elementary text-books on chemistry, the elements are grouped together with little regard to their analogies. Oxygen, hydrogen, and nitrogen are usually placed first, and therefore together, although there are hardly to be found three elements more dissimilar; again, phos-

* Communicated to the American Academy, Boston, Feb. 28, 1854.

phorus and sulphur, which are not chemically allied, are frequently placed consectively, while arsenic, antimony, and bismuth in spite of their close analogies with phosphorus, are described in a different part of the book. This confusion, which arises in part from retaining the artificial classification of the elements into metals and metalloids, is a source of great difficulty to the learner, since it obliges him to retain in his memory a large number of apparently disconnected facts. In order to meet this difficulty, a classification of the elements into six groups, differing but slightly from that given in the table accompanying this memoir, was made. The object of the classification was simply to facilitate the acquisition of chemistry, by bringing together such elements as were allied in their chemical relations considered collectively. As the classification has been in use for some time in the courses of lectures on chemistry given in Harvard University, I have had an opportunity for observing its value in teaching, and cannot but feel that the object for which it was made has been in a great measure attained. The series which is headed the Six Series will illustrate the advantage gained from the classification in a course of lectures, the elements which compose it being among those especially dwelt upon in lectures to medical students, and, generally, very widely separated in a text-book on the science. As chemistry is usually taught, the properties of the members of this series, nitrogen, phosphorus, arsenic, and antimony, as well as the composition and properties of their compounds, make up a large body of isolated facts, which, though without any assistance for his memory, the student is expected to retain. Certainly it cannot be wondered at, that he finds this a difficult task. The difficulty can, however, be in a great measure removed, if, after he has been taught that nitrogen forms two important acids with oxygen, $NO_3$ and $NO_5$, that it unites with sulphur and chlorine to form $NS_3$ and $NCl_3$, and also with three equivalents of hydrogen to form $NH_3$, he is also told, that, if in these symbols of the nitrogen compounds he replaces N by P, As, or Sb, he will obtain symbols of similar compounds of phosphorus, arsenic, and antimony; for he thus learns, once for all, the mode of combination of all four elements, so that when he comes to study the properties, in turn, of phosphorus, arsenic, and antimony, he has not to learn with each an entirely new set of facts, but finds the same repeated with only a few variations. Moreover, these very variations he will learn to predict, if he is shown that the elements are arranged in the series according to the strength of their electro-negative properties, or, in other words, that their affinities for oxygen, chlorine, sulphur, etc. increase, while those for hydrogen decrease, as we descend. He will then readily see why it is that, though nitrogen forms $NO_3$ and $NO_5$, it forms only $NCl_3$ and $NS_3$, and that this reason is correct he will be pleased to find

confirmed when he learns that phosphorus, which is more electropositive than nitrogen, and has, therefore, a stronger affinity both for chlorine and sulphur, forms not only $PCl_3$ and $PS_3$, but also $PCl_5$ and $PS_5$. Again, he will not be surprised, after seeing the affinity of the elements for hydrogen growing constantly weaker as he descends in the series, to learn that a compound of bismuth and hydrogen is not certainly known. Should he inquire why, though $NH_3$ has basic properties, $PH_3$, $AsH_3$, and $SbH_3$ have not, he can be shown that the loss of basic properties in passing from $NH_3$ to $PH_3$ corresponds to a decrease in the strength of the affinity between the elements, and that if in $PH_3$, $SbH_3$, or $AsH_3$, atoms of methyle, ethyle, or other organic radicals analogous to hydrogen, are substituted for the hydrogen atoms, and more stable compounds thus obtained, strong bases are the result. The other series would afford similar illustrations, and, from my own experience, I am confident that no teacher who has once used a classification of the elements like that here proposed, would ever think of attempting to teach chemistry without its aid.

Classifications of the elements, more or less complete, have been given by many authors; but the fact that no one has been generally received, is sufficient to prove that they are all liable to objections, and would, indeed, also seem to show that a strictly scientific classification is hardly possible in the present state of the science. The difficulty with most of the classifications is, undoubtedly, that they are too one-sided, based upon one set of properties to the exclusion of others, and often on seeming, rather than real resemblances. This is the difficulty with the old classification into metals and metalloids, which separated phosphorus and arsenic, sulphur and selenium, because arsenic and selenium have a metallic lustre, while phosphorus and sulphur have not, though there could hardly be found another point of difference. For a zoölogist to separate the ostrich from the class of birds because it cannot fly, would not be more absurd, than it is for a chemist to separate two essentially allied elements, because one has a metallic lustre and the other has not. Yet it is surprising to see how persistently this classification is retained in every elementary work on the science; and if it is sometimes so far modified as to transfer elements analogous to selenium and arsenic to the class of metalloids, this is only acknowledging the worthlessness of the principle, without being willing to abandon it. If there were any fundamental property common to all the elements, the law of whose variation was known, this might serve as the basis of a correct classification. Chemistry, however, does not as yet present us with such a property, and we must, therefore, here, as in other sciences, base our classification on general analogies. The most fundamental of all chemical properties is, un-

doubtedly, crystalline form; but a classification of the elements based solely on the principles of isomorphism is defective in the same way as it is in mineralogy. It brings together, undoubtedly, allied elements, but it also groups with them those which resemble each other only in their crystalline form. The mode of combining seems to be also a fundamental property; but, like crystalline form, it would bring together in some instances elements differing very widely in their chemical properties. A classification of the elements which shall exhibit their natural affinities, must obviously pay regard to both of these properties. It must at the same time seek to group together isomorphous elements, and those which form analogous compounds. Moreover, in such a classification, other less fundamental properties must not be disregarded. There are many properties both physical and chemical, which, although they cannot be exactly measured, and are oftentimes difficult to define, (such properties as those by which a chemist recognizes a familiar substance, or a mineralogist a familiar mineral,) and which on account of their indefinite character cannot be used as a basis of classification, may, nevertheless, render important aid in tracing out analogies. Judging from such properties as these, chemists are generally agreed in grouping together carbon, boron, and silicon, although they cannot be proved to be isomorphous, and are not generally thought to form similar compounds.

It is, however, much easier to point out what a classification should be, than to make one which shall fulfil the required conditions. Indeed, as has been already said, past experience would seem to show that a perfect scientific classification of the elements is hardly possible in the present state of chemistry. At best, the task is attended with great difficulties, and it cannot be expected that these should be surmounted at once. The classification which is offered in this memoir will, undoubtedly, be found to contain many defects. If, however, it is but one step in advance of those which have preceded it, it will be of value to the science. It was originally made, as has already been said, simply for the purpose of teaching, and never would have been published had it not led to the discovery of the numerical relation between the atomic weights.

On turning to the table which accompanies this memoir, it will be seen that the elements have been grouped into six series. These correspond entirely to the series of homologues of organic chemistry. In the group of volatile acids, homologues of formic acid, for example, we have a series of compounds yielding similar derivatives, and producing similar reactions, and many of whose properties, such as boiling and melting points, specific gravity, etc., vary as we descend in the series according to a determinate law. From formic acid, a highly limpid, volatile, and corrosive fluid,

the acids become less and less volatile, less and less fluid, less and less corrosive; first oily, then fat-like, and finally hard, brittle solids, like wax. As is well known, the composition of these acids varies in the same way, and the variation follows a regular law, so that by means of a general symbol we can express the composition of the class. This symbol for the volatile acids may be written $(C_2H)O_3$, $HO+n(C_2H_2)$.

This description of the well-known series of the volatile acids, applies, word for word, *nominibus mutatis*, to each of the six series of chemical elements. The elements of any one series form similar compounds and produce similar reactions; moreover, they resemble each other in another respect in which the members of the organic series do not. Their crystalline forms are the same, or, in other words, they are isomorphous. Although this may be true of the volatile acids, yet it cannot be proved in the present state of our knowledge. Still further, many of their properties vary in a regular manner as we descend in the series. In one case, at least, the law of the variation is known, and can be expressed algebraically, though in most instances it cannot be determined. Finally, as one general symbol will express the composition of a whole organic series, so a simple algebraic formula will express the atomic weight, or, if you may please so to term it, the constitution of a series of elements.

These points may be illustrated with any of the series in the table; with the first, for example, which consists of oxygen, fluorine, cyanogen, chlorine, bromine, and iodine. All these elements form similar compounds, as will be seen by inspecting the symbols of their compounds given at the right hand of the list of names, where the similar or homologous compounds are arranged in upright columns. Moreover, they are all isomorphous, as may be seen by referring to the left-hand side of the list, where the similar compounds in each upright series are isomorphous, the numbers at the heads of the columns indicating the systems of crystallization, as described in the explanation accompanying the table. That the properties of these elements vary as we descend, can be easily shown. Oxygen is a permanent gas, as is also fluorine. Cyanogen is a gas, but may be condensed to a liquid. Chlorine, a gas also, can be condensed more easily than cyanogen. Bromine is a fluid at the ordinary temperature; and, finally, iodine is a solid. Moreover, starting from cyanogen, the solubility of these elements in water decreases as we descend in the series; and, again, the specific gravity of their vapors follows the inverse order of progression, gradually increasing from oxygen down. The atomic weights vary in the same order, and admit of a general expression, which is $8+n9$, or, in other words, the differences between the atomic weights of these elements are always a multiple of nine. This general formula may be said to repre-

sent the constitution of these elements, in the same way that the symbol $(C_2H)O_3$, $HO+n(C_2H_2)$ represents the composition of the volatile acids before mentioned. In the place of $(C_2H)O_3$, HO we have $8=O=$ the weight of one atom of oxygen, and in the place of $C_2H_2$ we have nine. What it is that weighs nine (for it must be remembered that those numbers are weights) we cannot at present say, but it is not impossible that this will be hereafter discovered. In order to bring the general symbol of the volatile acids into exact comparison with that of the Nine Series, we must reduce the symbols to weights, when the two formulæ become

$46+n14$, where $46=(C_2H)O_3, HO$ and $14=C_2H_2$;
and $8+n\ 9$, where $8=O$ and $9=x$.

The numbers 46 and 14 are known to represent the weights of aggregations of atoms. The number 8 represents the weight of one oxygen atom, but we cannot as yet say what the 9 represents. After this comparison, it does not seem bold theorizing to suppose that the atoms of the members of this series are formed of an atom of oxygen as a nucleus, to which have been added one or more groups of atoms, the weight of which equals nine, or perhaps one or more single atoms each weighing nine, to which the corresponding element has not yet been discovered. As it will be convenient to have names to denote the two terms of the formulæ which represent the constitution of the different series, we will call the first term, in accordance with this theory, the nucleus, and the number in the second term multiplied by $n$ the common difference of the series.

From what has been said, it will be seen that the idea of the classification is that of the organic series. It is in this that the classification differs from those which have preceded it. Other authors, in grouping together the elements according to the principles of isomorphism, have obtained groups very similar to those here presented. Indeed, this could not be otherwise, since, as has been already said, the members of each series are isomorphous, while, as a general rule, to which, however, there are many exceptions, no isomorphism can be established between members of different series. These groups, however, have been merely groups of isomorphous elements, and not series of homologues like those in which the elements are here classed.

These general remarks will suffice to indicate the principles upon which the classification has been made, and the character of the numerical relation between the atomic weights which has been established. The details of the classification can be best studied by referring to the table, so that it will be only necessary to speak of those points which are of special interest, or which may require explanation, or in regard to which there may be doubt. The series I have named from their common differences.

The first I have called the Nine Series, the second the Eight Series, &c. Let us examine the doubtful points in each, commencing with the first.

The last five members of the Nine Series are connected by so many analogies, that they have been invariably grouped together in the elementary books. There can be no doubt, therefore, in regard to the propriety of placing them in the same series, on the ground of general analogies. Fluorine, it is true, presents some striking points of difference from the rest. Fluorid of calcium is almost insoluble in water, while the chlorid, bromid, and iodid of calcium are all very soluble. We must, however, remember that we have to do with series, and must not therefore expect to find close resemblances except between adjacent members. If, then, we consider that oxygen is one of the series, and that fluorine stands but one step removed from oxygen, while it is two steps removed from chlorine, the discrepancy in a measure vanishes, for lime CaO is but slightly soluble in water. Nevertheless, the difficulty does not entirely disappear; for CaFl is much less soluble than CaO, although it should be more soluble judging from the law of the series and the fact that CaCl is so much more soluble than CaFl.

The solubility of a series of homologous elements or compounds in water, may be regarded as a function of one or more variables. In the case of the elements there may be but one variable, but it is easy to see that in the case of compounds there must be several. One of these variables is probably the same which determines the common difference of the series to which the elements or compounds belong; (it will be hereafter shown that the atomic weights of the homologous compounds are related in the same way as those of the elements;) the other variables are perhaps the atomic forces which determine the hardness, density, &c. of the solid. We may, therefore, with justice, compare the relative solubilities of a series of homologues to a curve which should be the same function of the same variables, and what mathematics teaches we ought reasonably to expect in the case of this curve, we ought to expect also in the variations of solubility of these substances. Now every mathematician is familiar with the remarkably rapid changes which a curve undergoes that is a function of several variables, and we cannot be surprised that similarly rapid changes should be observed in the solubility of homologous substances in passing from one to the next in the series. In the curve which corresponds to the relative solubility of CaO, CaFl, CaCy, CaCl, CaBr, and CaI, it would seem that at CaFl there is a singular point where the curve, after rising for some distance above the axis, bends down again towards it. Several of the other series of compounds of these elements present similar anomalies; for example, KO, KFl, KCy, KCl, KBr, and KI.

Here the solubility diminishes until we come to KCl, which is less soluble than KCy; then it increases to the last. Here, of course, the singular point is at KCl. With the corresponding compounds of sodium, the solubility diminishes to NaFl, which is the least soluble of the series, and then increases constantly to the end.

These facts at least seem to show that apparent variations from the law of series in properties, which evidently are unknown functions of several variables, should not be allowed to outweigh strong analogies, and certainly the analogies between Fluorine and the other haloids are very marked. Fluorine itself possesses properties such as we should expect to find in a member of the series above chlorine. The strong and active affinities of fluorine might be indeed predicted, after seeing the rapid increase both in the strength and activity of the affinities in passing from iodine to chlorine. In passing from bromine to chlorine, we pass from a liquid to a gas, permanent under any natural conditions; and we should expect, therefore, in rising still higher in the series, to find in fluorine a gas less easily reduced to a liquid than chlorine. Now although, on account of its remarkably active affinities, this fact cannot be demonstrated on the gas itself, it can, nevertheless, be inferred with perfect certainty from its compounds. Finally, the isomorphism of fluorine and the other haloids may be urged as indicating close analogy. From these considerations, I cannot but think that those chemists who have questioned the propriety of classing fluorine with the other haloids will, on reviewing the facts, and regarding the haloids in the light of a series, and not simply as a group of elements possessing certain general properties, be led to change their opinion.

Cyanogen, though a compound radical, has been classed with the other haloids, not only from its atomic weight, but also from its other analogies. Its properties are in most cases those which we should expect from an element occupying its position in the series; but in others it presents remarkable variations, owing probably to the fact that it contains a radical which is easily decomposed. As is well known, it is perfectly isomorphous with chlorine.

The propriety of classing oxygen in this series seems to be placed beyond doubt by the discovery of ozone, which, although it does not seem to possess such energy as we should expect in an element higher in the series than fluorine, may, nevertheless, be found to fulfil all anticipations should it ever be obtained in a perfectly unmixed condition. The isomorphism of oxygen with chlorine, and therefore with the other haloids, seems sufficiently established by the determination both of Proust and Mitscherlich of the tetrahedral form of $Ca_2Cl$. It must, however, be admitted that oxygen presents as strong analogies with sulphur as it does

with chlorine; and since, not only from its analogies, but also from its atomic weight, it appears to be the nucleus in all the first three series, I have placed it at the head of each. It may be mentioned here, that in all cases the fact that the atomic weight of an element is included in the general formula of a series, is an argument for classifying it in that series, if the relation between the atomic weights pointed out in this memoir is admitted to be a law of nature; but as I wish to show that the relation is not that of a mere accidental group of numbers, but is connected with the most fundamental properties of the elements, and has, therefore, the claims of a law, I have endeavored to show the correctness of the classification which conforms to the law, and, indeed, in fact suggested the law on other grounds.

The atomic weights of the numbers of the Nine Series, as determined by experiment, present greater deviations from the numerical law already explained, than are to be found in any of the others. The weights which would exactly conform to the general formula $8+n9$ are given in the column of the table headed Theoretical, while in the next column at the right are given the weights of experiment. These for the most part (in this as well as in the other series), have been taken from the table of Atomic Weights given in the last volume of Liebig and Kopp, *Jahresbericht* for 1852, which was supposed to give the latest and most accurate results. In the few cases in which the numbers have not been taken from this table, the initial letter of the name of the observer has been annexed. It will be seen, on comparing the two columns that the greatest deviation from the law is in the case of fluorine, if we consider the care which was taken both by Berzelius and Louyet in the determination of the atomic weight of this element. It may, however, be remarked, that, as the processes used by both experimenters were essentially identical, any hidden constant source of error would produce the same effect on both results; so that the atomic weight of fluorine cannot be regarded as yet absolutely fixed. Nevertheless, it is not possible that so great a difference between the true and observed weights as two units could have escaped detection in the numberless analyses which have been made, by the most experienced chemists, of the fluorine compounds. It must, therefore, be admitted, and not only in the case of fluorine, but also in other instances, that there are deviations from the law; but these deviations are not greater than those from similar numerical laws in astronomy and other sciences, and indeed, judging from the analogy of these sciences, they ought to be expected.

Those who are not familiar with the amounts of probable error in the determination of the different atomic weights would judge, on comparing together the columns of theoretical and observed values, that the deviations from the law were much greater than

they are in reality. It should, therefore, be stated, that, in by far the larger number of instances, the deviations are within the limit of possible errors in the determinations, leaving only a few exceptional cases to be accounted for. It must be remembered that, other things being equal, the amount of probable error is the greater the greater the atomic weight, so that a difference of 1·9 in the case of iodine is not a greater actual deviation from the law than only 0·5 in the case of chlorine. Indeed, it is very possible that on more accurate determinations the atomic weight of iodine will be found to correspond to the law, which cannot be expected of that of chlorine. It is well known that many of the larger atomic weights, especially those of the rarer elements, cannot be regarded as fixed within several units.

I have calculated, as well as the data I have would permit, the amount of probable error in the determinations of many of the atomic weights, and by comparing the results from different processes, and by different experimenters, I have endeavored to detect the existence of constant errors, which seem to be the great errors in all these determinations, those accidental errors which are made in the repetitions of the same process by equally careful experimenters being comparatively insignificant. The results of this investigation will be published in a subsequent memoir. It is sufficient for the present purpose to state, that, while they show that, in the greater number of cases, the apparent variations from the law are within the limit of probable error, there are yet several instances, where, after allowing for all possible errors of observation, there is a residual difference. I do not therefore look alone to more accurate observations for a confirmation of the law, but, regarding the variations as ascertained facts, hope that future discovery will reveal the cause. Whether the variations will be found to be a secondary result of the very cause which has determined the distribution of the atomic weights according to a numerical law, as the perturbations in astronomy are a necessary consequence of the very law they seemed at first to invalidate, or whether they are due to independent causes, can of course, for the present, be only a matter of speculation. There are, however, facts which seem to indicate that the variations are not matters of chance, but correspond to variations in the properties of the elements.

From the beautiful discovery of Professor Schönbein we have learnt that oxygen has two allotropic modifications, and that besides its ordinary condition, it is capable of assuming another highly active state when its properties resemble those of chlorine. Cyanogen is known only in a quiescent state. The other haloids, fluorine, chlorine, bromine, and iodine, are known only in a highly active state. Now it will be seen on examining the table, that the atomic weights of the highly active elements, as determined

by experiment, exceed slightly the theoretical numbers, and that where the affinities are the most intense, in fluorine, the deviation is the greatest. A similar fact may be observed in the atomic weights of the members of the Six Series. Arsenic has been proved to be capable of existing in two allotropic modifications. In its ordinary state, it has a crystalline form belonging to the rhombic system. In the other condition, in which it may be obtained by sublimation at a low temperature, it crystallizes in regular octahedrons. The other members of this series are probably isodimorphs with arsenic. The ordinary condition of phosphorus is its monometric modification, while the rhombic state seems to be the normal condition of arsenic, antimony, and bismuth. Now the atomic weights of the last three are either equal to, or slightly exceed, the theoretical number, while that of the first falls short, perhaps even by a unit. Other facts, which also tend to show that the deviations are not matters of chance, may be found in the affiliations of the series. There are some elements which seem to be most remarkably double-faced, having certain properties which connect them closely with one series, and at the same time others which unite them nearly as closely to another. In such cases we find that the atomic weight either falls naturally into both series, or, not corresponding exactly with the theoretical number of the series to which the element properly belongs, it inclines towards that of the other, and sometimes equals it. Such is the case with chromium, manganese, and gold, as will be seen by referring to the affiliations at the bottom of the Nine Series. These various facts force upon me the conviction, that this relation between the atomic weights is not a matter of chance, but that it was a part of the grand plan of the Framer of the universe, and that in the very deviations from the law, there will, hereafter, be found fresh evidence of the wisdom and forethought of its Divine Author.

The general formulæ for the Eight Series are, $8+n8$ and $4+n8$. The two nuclei correspond to two different sets of elements, or sub-series, one consisting of oxygen, sulphur, selenium, and tellurium, the other of molybdenum, vanadium, tungsten, and tantalum. The atomic weights of the first are all equal to $8+n8$; those of the second to $4+n8$. The sub-series exhibit marked analogies, as well as certain differences. They resemble each other chiefly in that the members of both form analogous acids with oxygen, while they differ in that though the members of the first sub-series form compounds with hydrogen, those of the second do not. The isomorphism of the members of each sub-series among themselves, with the exception of vanadium, is complete; but there seems to be no proof of any isomorphism between the sub-series. Johnston attempted to establish the isomorphism of chromic and molybdic acids from the red variety of

molybdate of lead from Rezbanya, which he supposed to be a chromate; but the fact has been disproved by G. Rose, who has shown that the supposed chromate is a molybdate mixed with a small amount only of chromate. There seems, nevertheless, to be some reason for believing that chromic acid may replace molybdic acid to a certain extent. If this is proved, it establishes another link of connection between the members of the two sub-series, since chromic acid is isomorphous with sulphuric acid. For the present, however, we must regard them as sub-series, related, but distinct, the second being in a measure supplementary to the first. They are distinguished in the table by printing the names of the second sub-series a little to the right of those of the first, and the fact that their atomic weights are intermediate to those of the first, I have indicated to the eye by giving to the names also an intermediate position.

The analogies between oxygen and sulphur are so numerous, that, were we to place oxygen in but one series, we should place it in this. HO and HS, $HO_2$ and $HS_2$, resemble each other very closely, as do also the oxygen salts the corresponding sulphur salts. Moreover, there can be no doubt in regard to the isomorphism of the two elements, since it has been established upon the authority both of Mitscherlich and Becquerel, and from two different compounds. The only doubtful case in the series was that of vanadium, which in some of its properties resembles arsenic more closely than it does molybdenum. The reasons for giving it the place which it occupies were the facts that its acids correspond to those of molybdenum, and that it forms remarkably highly colored oxyds which are repeated also in molybdenum. It is true that the properties of the element itself are not those we should expect from the position which it occupies in our table; yet, if it were placed in the Six Series, it would fall between phosphorus and arsenic, which on the whole it resembles less than it does molybdenum, for although it is combustible, yet neither it nor its oxyds are volatile. I consider it, therefore, as a member of the Eight Series, but affiliating very closely with the Six. Its atomic weight favors this hypothesis. Vanadate of lead has been considered isomorphous with the phosphate; but as this isomorphism does not rest on any measurement of angles, and as, moreover, the received symbols of the two minerals, vanadinite and pyromorphite, on whose crystalline forms the isomorphism was determined, show a very different constitution, I have not given much weight to this fact.* The observed atomic weights of the members of this series are almost precisely the same as the theoretical members, and with the exception, perhaps, of molybdenum, there appears to be no instance in which the difference is greater than the amount of possible error.

* See G. Rose's Mineral System.

The members of the Six Group form a well characterized family, so that, with the exception of oxygen, there can be no doubt in regard to the justice of classifying them together, and any discrepancies will disappear on considering the group in the light of a series. They form acids containing three and five atoms of oxygen which are completely homologous, and make two series parallel to that of the elements. They form also a remarkable series of compounds with three atoms of hydrogen. The idea which has been advanced by some authors, that $NH_3$ is the nitrid of hydrogen, while $PH_3$ is the hydruret of phosphorus, or, in other words, that hydrogen is electro-positive with reference to nitrogen and electro-negative with reference to phosphorus and those lower in the series, does not seem to me correct, since the remarkable bases which may be formed from $PH_3$, $AsH_3$, $SbH_3$, and $BiH_3$, by replacing the hydrogen atoms by organic radicals, seem to indicate that they have the same type as $NH_3$, and are therefore homologues of it.

The isomorphism of the four lower members of the series is perfect. It has been shown in the table, both by the crystalline forms of the elements themselves, as well as by those of their compounds. In the other series, wherever it was possible, the same double proof has been given. The doubt expressed by G. Rose in regard to the dimorphism of arsenic, as I hope to be able to show in a paper soon to be published, has been removed. In one state arsenic crystallizes in perfect octahedrons of the regular system, and is therefore isomorphous, not only with antimony and bismuth, but also, in its allotropic state, with phosphorus. Isomorphism, as is well known, is not absolute, except in forms of the regular system. The rhombic angles of the crystals of arsenic, antimony, and bismuth, are respectively, 85° 41′, 87° 35′, 87° 40′, and therefore conform to the general rule. It will be observed that the angle varies constantly in the same way as we descend in the series. Now, although these few instances do not afford sufficient ground for any general conclusion, yet they show that similar variations are possible in the other systems, and therefore that we cannot be expected to establish isomorphism in any case except between merely consecutive members.

The atomic weights of the members of this series, with the exception of phosphorus, do not present any important deviations from the theoretical numbers, taking into account always, of course, the amount of possible error. The deviation in the case of phosphorus has already been noticed. Oxygen, it must be admitted, is not connected with the series from any similarity of properties though the phosphids, arsenids, and antimonids, present certain analogies with the oxyds. As has already been said, oxygen was placed at the head of this, as well as of the last two series, because its atomic weight seemed to be the nucleus of all three.

The Five Series is the shortest of all, consisting of only three members, carbon, boron and silicon. Of these, the last two are as closely allied as are any two members of the other series, silicon having precisely the properties we should expect in a homologue of boron, which was lower in the series; and the same is also true of their compounds. The analogies, however, between these two elements and carbon are by no means so close, for not only carbon cannot be proved to be isomorphous with them, but it does not form similar compounds. Carbonic acid, it is true, presents some points of resemblance to boracic and silicic acid; like them it unites in a large variety of proportions with bases, its alkaline salts give a basic reaction, &c.; but according to the generally received opinion, its symbol is $CO_2$, while those of boron and silicon are $BO_3$ and $SiO_3$. In its uncombined state, however, carbon resembles boron and silicon, not only in its outward properties, but also in its action before the blowpipe. Two of the allotropic states of carbon, graphite and charcoal, are probably repeated in boron, and are known to be in silicon. The principle of exclusion would also seem to place carbon in this series, for it certainly presents no analogies with the members of any other. The correspondence of the atomic weights of the members of this series to the law is remarkably close.

The four series is by far the largest of all, including the greater number of what are generally known as the heavy metals. The members of the series resemble each other in the following respects. First, they are isomorphous; for although each member cannot be directly proved to be isomorphous with every other, yet isomorphism can be established between consecutive members, which, as has before been said, is all that can be expected. Second, the members of this series all form, by uniting with oxygen, either protoxyds or sesquioxyds, or both, which, as a general rule, are strong bases. Third, these oxyds are either insoluble, or nearly insoluble, in water. And finally, the elements of the series have all those physical properties which are known as metallic properties.

This series may be naturally divided into two sub-series. The first contains those elements whose protoxyd bases are their characteristic compounds, and which do not form acids with oxygen. The second contains those elements whose characteristic compounds are their sesquibases. They generally unite with two or more equivalents of oxygen, and form acids. These sub-series are distinguished in the table in the same way as those of the Six Series. Corresponding to these sub-series we have two sets of atomic weights, each having the same common difference, but differing in their starting-point or nucleus. The first set is expressed by the formula $4+n4$, the second by $2+n4$.

The sub-series affiliate with each other in a most remarkable manner. Manganese, for example, not only forms a strong protoxyd base, but also unites with a larger amount of oxygen, forming both a sesquibase and acids. Its atomic weight places it in the first group, and it has therefore been classed there, although by its properties it is equally allied to the second. Cobalt and nickel certainly resemble much more closely the members of the first than of the second sub-series, although their atomic weights place them in the second. With this exception, the subdivision of the series which the atomic weights require does not differ from that suggested by the properties of the elements. The members of this series may of course be still further subdivided into groups according to their special properties, as they are in all works on chemistry. They are placed together here because the atomic weights form but one numerical series.

The isomorphism of the members of this series will be found well established with the limitations before given. In order to establish the isomorphism of cobalt and nickel with iron, the isomorphism of one atom of arsenic with two atoms of sulphur has been assumed. This is generally admitted; but if it is not, no one can doubt in regard to the isomorphism of these three metals, as they constantly replace each other. Glucinum, zirconium, lanthanum, cerium, and thorium, cannot be shown to be isomorphous with the other metals by any of their compounds, but their oxyds are known to replace the analogous oxyds of the other metals. So also is ruthenium known to replace rhodium. There have been doubts expressed in regard to the existence of a monometric form of zinc; but as we have established its isomorphism with the other members of the series, not only by its own crystalline form, but also by those of its compounds, the fact is of no importance to the present question.

The atomic weights of the members of this series, as determined by observation, very nearly correspond with the theoretical numbers, which is the more remarkable, as the limit of error in the determination of the atomic weights of the greater number, especially of the rarer metals, is quite wide.

The Three and last Series is composed of hydrogen and the metals of the alkalies. The analogies between lithium, sodium and potassium, are very close, as is well known, and there can be no doubt in regard to the propriety of classing them together. It may be said however, in regard to hydrogen, that it resembles as closely some of the metals of the four series as it does those of the alkalies. Though this cannot be denied, yet the fact that the atomic weight of hydrogen is the nucleus of the series, and the great solubility of the alkalies in water, may be urged as reasons for placing it at the head of the Three Series.

The isomorphism of lithium, sodium, and potassium, is fully established; but I can find no data which prove hydrogen isomorphous either with them or with the metals of the other group.

The unit of the atomic weights which has been used thus far throughout the table, is the double atom of hydrogen; but the nucleus of the Three Series is the weight of the single atom, so that the unit in this series is one half of the unit of the weights in all the other series. This fact must be kept in mind in comparing the atomic weights of this with those of the other series. All the weights might have been made uniform by doubling them throughout; but as this would not have changed the relation, and would have been departing from the general custom, it was thought best to confine the doubling to the Three Series into which alone hydrogen enters. The general symbol of this series is $1+3n$, where of course the unit is one half of that of the symbols at the head of the other series. The observed atomic weights will be found to correspond very closely with the theoretical numbers; indeed, the two coincide, except in the case of potassium, where the difference is 0·6. This, however, it must be remembered, is 0·6 of the single hydrogen atom. Compared with the double atom, as the weight of potassium is generally given, the difference amounts to but 0·3.

One of the most remarkable points of the classification which has been now explained, is the affiliation of the series. We find in chemistry, as in other sciences, that Nature seems to abhor abrupt transitions, and shades off her bounding lines. Many of the elements, while they manifestly belong to one series, have properties which ally them to another. Several examples of this have already been noticed. In such cases, we find invariably, that there is a similar affiliation of the atomic weight. Of all the elements chromium and manganese are the most protean. Two atoms of these elements unite with seven atoms of oxygen and form acids analogous to perchloric acid, and, as has already been shown, the weight of two atoms of either element falls into the Nine Series. Moreover, one atom of chromium or of manganese, unites with three atoms of oxygen, to form chromic or manganic acid. Chromic acid is a strong oxydizing agent, and resembles closely nitrous acid, and the atomic weight of chromium falls into the Six Series just below that of nitrogen. Manganic acid, on the other hand, resembles sulphuric acid, with which it is isomorphous, and the atomic weight of manganese would place it in the Eight Series. In like manner osmium in many of its properties resembles platinum and the other metals with which it is associated in nature; but, unlike them, it forms a very remarkable volatile acid, whose insupportable and suffocating odor, as well as composition, reminds one of the acids of the Nine Series, and its atomic weight seems to justify the apparent analogy. Gold

likewise, though the noblest of metals, yet in some of its chemical relations resembles much more closely the members of the Nine than of the Four Series, and here again its accommodating atomic weight seems to account for its double-sided character. Several other examples of similar affiliations are given in the table, but do not need explanation.

In the description just concluded of the classification of the chemical elements, which is offered in this memoir, I have not entered into details, for to have done so would have been to write a treatise on chemistry. I have confined myself almost exclusively to general points, and referred only to those particulars which I thought might present doubts. I hope that I have been able to show, first, that the chemical elements may be classified in a few series similar to the series of homologues of organic chemistry; second, that in those series the properties of the elements follow a law of progression; and finally, that the atomic weights vary according to a similar law, which may be expressed by a simple algebraic formula. As already intimated, I have endeavored to prove the correctness of the classification on general grounds, in order that it might appear that the simple numerical relation which has been discovered between the atomic weights is not a matter of chance, but is connected with the most fundamental properties of the elements. I might leave the subject at this point, but the existence of the law which I wish to establish will be proved more conclusively if it can be shown, not simply that the general properties of the members of each series vary in a regular manner, but also if in one or more cases the exact law of the variation can be pointed out.

There are but few properties of the elements which are subjects of measurement, and which therefore can be compared numerically. Such are the specific gravity in which the three states of aggregation, the boiling and melting points, the capacity of heat, and a few others. It is easy to see that there are but few of these properties the law of whose variation in the series we could reasonably expect to discover in the present state of science. Most of them evidently depend upon molecular forces with which we are entirely unacquainted. Such in solids is undoubtedly the case with so simple and fundamental a property as specific gravity, and most, if not all, of the other properties of solids belong to the same category. It cannot therefore be expected that we should point out the laws by which these properties vary, although the remarkable investigations of Kopp, Dana, Filhol, Schröder, and others, on the relations between the density of substances and their atomic weights, and those of Kengott on the relation of hardness to atomic volume, give grounds for expecting that even they will before long be discovered. In liquids and gases, however, most of these molecular forces which produce the ap-

parent irregularities in solids have less influence, as we should naturally expect, probably because the atoms are removed out of the sphere of their action. We may therefore hope, on comparing together the properties of the liquid or gaseous states of the elements in any series, to discover some numerical relation between them. Unfortunately, however, we have not sufficient data for making such a comparison except in the case of one property, the specific gravity. The boiling point, which would be a very valuable property for the purpose, is known only in a few instances.

That the specific gravity of the elements in their gaseous state varies in each series according to a numerical law, follows necessarily from what is already known. It is a well-known fact, that the specific gravities of the gaseous states of the elements divided by their atomic weights give quotients which are either equal, or which stand in a very simple relation to each other. For any series, as far as we have data, this quotient is the same for all the elements with only a few exceptions. That is $\frac{\text{Sp. Gr.}}{\text{At. W.}} = p$. But we have found that At. W. may be expressed in general by $a+nb$, and substituting this for for At. W. in the above equation, it becomes $\frac{\text{Sp. Gr.}}{a+nb} = p$, or sp. gr. $= pa + npb$; so that $pa + npb$ is a general expression for the specific gravity of all the elements of any series, in the same way that $a + nb$ is for the atomic weight. The value of $p$ will differ according as the specific gravities used are referred to hydrogen or air. Below will be found tables which give the calculated and observed specific gravities of the elements of the Nine and Six Series referred to hydrogen, which has been taken as the unit instead of air, as we thus in a great measure avoid fractions. In the Nine Series $p=1$, so that the numbers representing the specific gravities are the same as those representing the atomic weights. In the Six Series it equals two, so that the numbers representing the specific gravities are in this series twice as large as those representing the atomic weights. When the specific gravity has not been observed, the calculated number only is given. The observed numbers are taken from the "Table of Specific Gravity of Gases and Vapors," in Graham's *Elements of Chemistry*, which is a very complete collection of all known data. For the other series, we have only occasional data, so that no complete tables of their specific gravities are possible.

It is evident, then, that at least one property of the elements varies in the series according to an ascertained numerical law. But, it may be said, this proves nothing, for these specific gravities are connected so closely with the atomic weights that what is true of one must be to the same extent true of the other. It must be remembered, however, that the specific gravities are a distinct set of observed facts, and that the probability of a law is

in exact proportion to the number of facts which accord with it. Moreover, the closeness of the connection is unimportant. Whether the value of $p$ be expressed by a single digit, or by a complicated algebraic formula, is evidently a matter of indifference so far as the confirmation of the law is concerned.

| THE NINE SERIES. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=1.$ Sp. Gr. $=8+n9$. | | | THE SIX SERIES. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=2.$ Sp. Gr. $=16+n12$. | | |
|---|---|---|---|---|---|
| Names. | SPECIFIC GRAVITIES. | | Names. | SPECIFIC GRAVITIES. | |
| | Theoret. | Observed. | | Theoret. | Observed. |
| Oxygen | 8 | 16 | Oxygen | 16 | 16 |
| Fluorine | 17 | | Nitrogen | 28 | 14 |
| Cyanogen | 26 | 26 | Phosphorus | 64 | 64 |
| Chlorine | 35 | 35·5 | Arsenic | 148 | 150 |
| Bromine | 80 | 78 | Antimony | 256 | |
| Iodine | 125 | 126 | Bismuth | 412 | |

I regret exceedingly that there are not sufficient data in the case of any of the other properties of the elements in the state of gas to allow comparison, as I feel confident that the law which governs their variation in the series might easily be discovered; but I look forward to the time when in the general formula $pa+npb$ the value of $p$ shall be known, not only for the properties of the elements in their gaseous state, but for every property capable of numerical expression.

In this memoir I have confined myself entirely to the elements, but it is evident that the classification here offered, and the numerical law here explained, may be extended to all compounds. The elements of any one series, by combining, give rise to perfectly parallel series of homologous binaries, some of which are given in the table. The binaries of those series which have the greatest common difference are generally acids; and of those which have the smallest, they are generally bases. These acids and bases unite together and form series of homologous salts. As in organic chemistry, many of the series are very incomplete; but they are much more generally perfect than in that newer department of the science, and almost every day fills up some gap.

It will be seen, then, that not merely a plan has been given for classifying the elements, but one which will also embrace all inorganic compounds, and affiliate with the similar classification which has already been established in organic chemistry. We have not attempted to develop such a classification, since to do it would require a volume; nor is it necessary, as any one can develop it for himself.

That the atomic weights of the series of homologous compounds follow the same numerical law as those of the elements is easily shown. Take as an example the series of salts homologous with $KO, NO_5$, which may be expressed in general by

$KO, RO_5$, where R is any member of the Six Series after oxygen, and whose atomic weight, therefore, equals $8+n6$. The atomic weight of $KO, RO_5$ must be necessarily $39{\cdot}5+48+(8+n6)$, or $95{\cdot}5+n6$. As this symbol differs from that of the Six Series only in the nucleus, the atomic weights of the salts which are represented by it must progress by the same differences as those of the corresponding elements.

The properties of these series of homologous compounds will also be found to vary in a regular manner, and the law of the progression of the specific gravities in the gaseous state can be easily expressed algebraically, since in each series the quotient of the specific gravity divided by the atomic weight is a constant quantity. As an illustration, we may take the series of binaries homologues of water given in the Nine Series of our table. It follows from what has been said, that the atomic weights of these compounds equals $9+n9$. With each $\frac{\text{Sp. Gr.}}{9+n9}=\frac{1}{2}$, therefore Sp. Gr. $=4{\cdot}5+n4{\cdot}5$. We give below a table of the observed or calculated specific gravities, not only of these compounds, but also of those homologues of $NH_3$ whose specific gravity has been observed.

| HOMOLOGUES OF WATER. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=\frac{1}{2}$. Sp. Gr. $=4{\cdot}5+n4{\cdot}5$. | | | HOMOLOGUES OF AMMONIA GAS. $\frac{\text{Sp. Gr.}}{\text{At. W.}}=\frac{1}{2}$. Sp. Gr. $=5{\cdot}5+n3$. | | |
|---|---|---|---|---|---|
| Symbols. | SPECIFIC GRAVITIES. | | Symbols. | SPECIFIC GRAVITIES. | |
| | Theoret. | Observed. | | Theoret. | Observed. |
| HO | 4·5 | 9 | $NH_3$ | 8·5 | 8·5 |
| HFl | 9 | | $PH_3$ | 17·25 | 17·5 |
| HCl | 13·5 | 13·5 | $AsH_3$ | 39 | 38·5 |
| HBr | 40·5 | 39·5 | | | |
| HI | 63 | 63·5 | | | |

As the series of compounds give a greater scope for investigating the relations of properties than is presented by those of the elements, we may expect that these relations will be first discovered in the former, and to my conceptions chemistry will then have become a perfect science, when all substances have been classed in series of homologues, and when we can make a table which shall contain, not only every known substance, but also every possible one, and when by means of a few general formulæ we shall be able to express all the properties of matter, so that when the series of a substance and its place in its series are given, we shall be able to calculate, nay, predict, its properties with absolute certainty; and when our chemical treatises shall have been reduced to tables of homologues, and our laws comprised in a few algebraic formulæ, then the dreams of the ancient alchemist will be realized, for the problem of the transmutation of the elements will have been theoretically, if not practically, solved.

# THE NINE SERIES.

## $8 + n9$.

| ISOMORPHS. | | | | HOMOLOGUES. | | | | | | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 3 | | 1 | | | | | | | | | Theo. | Observed. | $n$ |
| | | *$Cu_2O$ | | Oxygen. | HO | | | | | | 8 | 8 | $n = 0$ |
| | | | KFl | Fluorine. | HFl | | | | | | 17 | 18.8B | $n = 1$ |
| | | | KCy | Cyanogen. | HCy | CyO | | | | | 26 | | $n = 2$ |
| $KO,ClO_7$ | | *$Cu_2Cl$ | KCl | Chlorine. | HCl | ClO | $ClO_3$ | $ClO_4$ | $ClO_5$ | $ClO_7$ | 35 | 35.5 | $n = 3$ |
| | | | KBr | Bromine. | HBr | BrO? | | | $BrO_5$ | | 80 | 80 | $n = 8$ |
| | | | KI | Iodine. | HI | IO | | | $IO_5$ | $IO_7$ | 125 | 126.9M | $n = 13$ |
| AFFILIATIONS. | | | | | | | | | | | | | |
| | $KO,CrO_3$ | | | Chromium. | | | | | | $Cr_2O_7$ | 53 | 53.4 | $n = 5$ |
| $KO,Mn_2O_7$ | $KO,MnO_2$ | | | Manganese. | | | | | | $Mn_2O_7$ | 53 | 55.2 | $n = 5$ |
| | | | | Osmium. | | | | $OsO_4$ | | | 98 | 99.4 | $n = 10$ |
| | | | | Gold. | | | $AuO_3$ | | $AuO_5$? | | 197 | 197 | $n = 21$ |

* Mitscherlich J. pr. Chem. 19, 449.

| 6 | 4 | |
|---|---|---|
| | | KO,SO |
| | | KO,SeO |
| Te | | |
| | $FeO,WO_3$ | |
| | $FeO,TaO_3$ | |
| As | | |
| | | KO,MnO |

* Mitscherlich J. pr. Ch

# THE EIGHT SERIES.

$8 + n8$ or $4 + n8$.

| ISOMORPHS. | | | | | HOMOLOGUES. | | | | | ATOMIC WEIGHTS. | | | ISOMORPHS. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 3 | | 2 | 1 | | | | | | | Theo | Observed. | $n$ | 6 | 3 | |
| | | | †PbO | *$Cu_2O$ | Oxygen. | HO | $HO_2$ | | | 8 | 8 | $n=0$ | | | |
| KO,$SO_3$ | ‡$Fe_2S_4$ | | PbS | *$Cu_2S$ | Sulphur. | HS | $HS_2$ | $SO_2$ | $SO_3$ | 16 | 16 | $n=1$ | | | |
| KO,$SeO_3$ | | | PbSe | | Selenium. | HSe | | $SeO_2$ | $SeO_3$ | 40 | 39.6B | $n=4$ | | $NaO,PO_5+4HO$ | |
| | | PbO,$MoO_3$ | | | Molybdenum | | | $MoO_2$ | $MoO_3$ | 44 | 46 | $n=5$ | | | |
| | | | PbTe | | Tellurium. | HTe | | $TeO_2$ | $TeO_3$ | 64 | 64.1B | $n=7$ | As | $NaO,AsO_5+4HO$ | |
| | | | | | Vanadium. | | | $VaO_2$ | $VaO_3$ | 68 | 68.5B | $n=8$ | | | |
| | | | | | | | | | | | | | Sb | | $SbS_3$ |
| | | PbO,$WO_3$ | | | Tungsten. | | | $WO_2$ | $WO_3$ | 92 | 92 | $n=11$ | | | |
| | | | | | | | | | | | | | Bi | | $BiS_3$ |
| | | | | | Tantalum. | | | $TaO_2$ | $TaO_3$ | 188 | 184 | $n=23$ | | | |

## AFFILIATIONS.

| | | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ‡$Fe_2(AsS_2)$ | | | | Arsenic. | | | | | 148 | 150 | $n=18$ | | | |
| KO,$MnO_3$ | | | | | Manganese. | | | | $MnO_3$ | 28 | 27.6 | $n=3$ | | | |

J. pr. Chem. 19, 449. † Becquerel Ann. Chem. Phys. 51, 105. ‡ G. Rose Krystallochemische Mineralsystem.

# THE EIGHT SERIES.

$8 + n8$ or $4 + n8$.

| | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|
| | | Theo. | Observed. | $n$ |
| | | 8 | 8 | $n = 0$ |
| | | 17 | 18 8B | $n = 1$ |
| | | 26 | | $n = 2$ |
| $ClO_5$ | $ClO_7$ | 35 | 35.5 | $n = 3$ |
| $BrO_5$ | | 80 | 80 | $n = 8$ |
| $IO_5$ | $IO_7$ | 125 | 126.9M | $n = 13$ |
| | $Cr_2O_7$ | 53 | 53.4 | $n = 5$ |
| | $Mn_2O_7$ | 53 | 55.2 | $n = 5$ |
| | | 98 | 99.4 | $n = 10$ |
| $AuO_5$ ? | | 197 | 197 | $n = 21$ |

| ISOMORPHS. | | | | | | | HOMOLOGUES. | | | | | ATOMIC W | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 6 | 4 | 3 | | 2 | 1 | | | | | | | Theo. | Observe |
| | | | | | †PbO | *$Cu_2O$ | Oxygen. | HO | $HO_2$ | | | 8 | 8 |
| | | $KO,SO_3$ | ‡$Fe_2S_4$ | | PbS | *$Cu_2S$ | Sulphur. | HS | $HS_2$ | $SO_2$ | $SO_3$ | 16 | 16 |
| | | $KO,SeO_3$ | | | PbSe | | Selenium. | HSe | | $SeO_2$ | $SeO_3$ | 40 | 39 6B |
| | | | | $PbO,MoO_3$ | | | Molybdenum | | | $MoO_2$ | $MoO_3$ | 44 | 46 |
| Te | | | | | PbTe | | Tellurium. | HTe | | $TeO_2$ | $TeO_3$ | 64 | 64.1B |
| | | | | | | | Vanadium. | | | $VaO_2$ | $VaO_3$ | 68 | 68.5B |
| | $FeO,WO_3$ | | | $PbO,WO_3$ | | | Tungsten. | | | $WO_2$ | $WO_3$ | 92 | 92 |
| | $FeO,TaO_3$ | | | | | | Tantalum. | | | $TaO_2$ | $TaO_3$ | 188 | 184 |

AFFILIATIONS.

| | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| As | | | ‡$Fe_2(AsS_2)$ | | | | Arsenic. | | | | | 148 | 150 |
| | | $KO,MnO_3$ | | | | | Manganese. | | | | $MnO_3$ | 28 | 27.6 |

* Mitscherlich J. pr. Chem. 19, 449. † Becquerel Ann. Chem. Phys. 51, 105. ‡ G. Rose Krystallochemische Mineralsystem.

$8 + n6$.

| ISOMORPHS. | | | | | HOMOLOGUES. | | | | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| G | 3 | | 1 | | | | | | | Theo. | Observed. | n |
| | | | | | Oxygen. | | | | | 8 | 8 | $n=0$ |
| | | | | | Nitrogen. | *$NH_2$ | $NH_3$ | $NO_3$ | $NO_5$ | 14 | 14 | $n=1$ |
| | | | | | | $PMe_2$ | $PMe_3$ | | | | | |
| | $NaO,PO_5+4HO$ | | | P | Phosphorus. | $PH_2$ | $PH_3$ | $PO_3$ | $PO_5$ | 32 | 31 | $n=4$ |
| | | | | | | $AsMe_2$ | | | | | | |
| As | $NaO,AsO_5+4HO$ | | $AsO_3$ | As | Arsenic. | | $AsH_3$ | $AsO_3$ | $AsO_5$ | 74 | 75 | $n=11$ |
| | | | | | | | $SbMe_3$ | | | | | |
| Sb | | $SbS_3$ | $SbO_3$ | | Antimony. | | $SbH_3$ | $SbO_3$ | $SbO_5$ | 128 | 129 | $n=20$ |
| | | | | | | | $BiAe_3$ | | | | | |
| Bi | | $BiS_3$ | | | Bismuth. | | $BiH_3$ ? | $BiO_3$ | $BiO_5$ | 206 | 208 | $n=33$ |

AFFILIATIONS.

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | Chromium. | | | $CrO_3$ | | 26 | 26.7 | $n=3$ |
| | | | | | Vanadium. | | | $VaO_3$ | | 68 | 68.5 | $n=10$ |

* Only known in combination.

## THE FIVE SERIES.

$6 + n5$.

| ISOMORPHS. | | HOMOLOGUES. | | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|---|---|---|
| | 1 | | | | Theo. | Observed. | n |
| | C | Carbon. | $CO_2$ | | 6 | 6 | $n = 0$ |
| | | Boron. | | $BO_3$ | 11 | 10.9 | $n = 1$ |
| | | Silicon. | | $SiO_3$ | 21 | 21.3 | $n = 3$ |
| AFFILIATIONS. | | | | | | | |
| | Ti | Titanium. | $TiO_2$ | | 26 | 25.2 | $n = 4$ |

| ISOMORPH | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 6 | | | | 5 | 3 | 2 | | |
| $MgO,CO_2$ | | | | | | | | |
| | $Al_2O_3$ | | | | | | | |
| $CaO,CO_2$ | | | | | $CaO,CO_2$ | | | |
| | $(TiFe)O_3$ $Cr_2O_3$ | | | | | $TiO_2$ | | |
| $MnO,CO_2$ $FeO,CO_2$ | $Fe_2O_3$ | | | $MnO,SO_3+5HO$ | | | | |
| $ZnO,CO_2$ | | | Zn | $CuO,SO_3+5HO$ | | | | |
| | | | | | $SrO,CO_2$ | | | |
| | | (Ir,Rh)Os | | | | | | |
| | | | Pa | | | | | |
| | | | | | | $SnO_2$ | KCl,Sn | |
| | | | | | $BaO,CO_2$ | | | |
| | | (Ir,Rh)Os | | | | | KCl,Ir<br>KCl,Os<br>KCl,Pt | |
| | | | | | $PbO,CO_2$ | | | |

# THE FOUR SERIES.

## $4 + n4$ or $2 + n4$.

| OMORPHS. | | | | | | | | | HOMOLOGUES. | | | | | | | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | | | | | | | | | | | | | | | | Theo. | Observed. | $n$ |
| | | | | | | | | | Glucinum. | | GlO | | | | | | 4 | 4.7 | $n=0$ |
| | | $MgO,Al_2O_3$ | | | | MgO | | | Magnesium. | | MgO | | | | | | 12 | 12 | $n=2$ |
| | $ZnO,Al_2O_3$ | | | | | | | | Aluminium. | | | $Al_2O_3$ | | | | | 14 | 13.7 | $n=3$ |
| | | | | | | | | | Calcium. | | CaO | | | $CaO_2$ | | | 20 | 20 | $n=4$ |
| | | | | | | | Ti | | Titanium. | | TiO | | | $TiO_2$ | | | 26 | 25.2B | $n=6$ |
| | | | | | | | | | Chromium. | | CrO | $Cr_2O_3$ | $Cr_3O_4$ | $CrO_2$ | $CrO_3$ | $Cr_2O_7$ | 26 | 26.7 | $n=6$ |
| | $FeO,Fe_2O_3$ | | $MnS_2$ | | MnS | | | | Manganese. | | MnO | $Mn_2O_3$ | $Mn_3O_4$ | $MnO_2$ | $MnO_3$ | $Mn_2O_7$ | 28 | 27.6 | $n=6$ |
| | | | $FeS_2$ | | | | Fe | | Iron. | | FeO | $Fe_2O_3$ | $Fe_3O_4$ | $FeO_2$ | | | 28 | 28 | $n=6$ |
| | | | $CoS_2+CoAs$ | | | | | | Cobalt. | | CoO | $Co_2O_3$ | $Co_3O_4$ | | | | 30 | 29.5R | $n=7$ |
| | | | $NiS_2+NiAs$ | | | | | | Nickel. | | NiO | $Ni_2O_3$ | | | | | 30 | 29.5R | $n=7$ |
| | | | | | $Cu_2S$? | $Cu_2O$ | Cu | | Copper. | $Cu_2O$ | CuO | | | | | | 32 | 31.6B | $n=7$ |
| | $ZnO,Fe_2O_3$ | $ZnO,Al_2O_3$ | | | ZnS | | Zn? | | Zinc. | $Zn_2O$ | ZnO | | | | | | 32 | 32.5Ird. | $n=7$ |
| | | | | | | | | | Zirconium. | | | $Zr_2O_3$ | | | | | 34 | 33.6B | $n=8$ |
| | | | | | | | | | Strontium. | | SrO | | | $SrO_2$ | | | 44 | 43.8 | $n=10$ |
| | | | | | | | | | Lanthanum. | | LaO | | | $LaO_2$ | | | 48 | 47 | $n=11$ |
| | | | | | | | | | Cerium. | | CeO | $Ce_2O_3$ | | | | | 48 | 47.3M | $n=11$ |
| | | | | | | | | | Rhodium. | | RO | $R_2O_3$ | $R_3O_4$ | | | | 52 | 52.1B | $n=12$ |
| | | | | | | | | | Ruthenium. | | RuO | $Ru_2O_3$ | | $RuO_2$ | $RuO_3$? | | 52 | 52.1B | $n=12$ |
| | | | | | | | Pa | | Palladium. | | PdO | | | $PdO_2$ | | | 54 | 53.3 | $n=13$ |
| | | | | | | | Cd | | Cadmium. | $Cd_2O$ | CdO | | | | | | 56 | 56 | $n=13$ |
| $KCl,SnCl_2$ | | | | | | | Sn | | Tin. | | SnO | $Sn_2O_3$ | | $SnO_2$ | | | 58 | 58 | $n=14$ |
| | $UrO,Ur_2O_3$ | | | | | | | | Thorium. | | ThO | | | | | | 60 | 59.6 | $n=14$ |
| | | | | | | | | | Uranium. | | UrO | $Ur_2O_3$ | $Ur_3O_4$ | | | | 60 | 60 | $n=14$ |
| | | | | | | | | | Barium. | | BaO | | | $BaO_2$ | | | 68 | 68.5 | $n=16$ |
| $KCl,IrCl_2$ | | | | | | | Ir | | Iridium. | | IrO | $Ir_2O_3$ | | $IrO_2$ | $IrO_3$ | | 98 | 98.5B | $n=23$ |
| $KCl,OsCl_2$ | | | | | | | | | Osmium. | | OsO | $Os_2O_3$? | | $OsO_2$ | $OsO_3$ | | 98 | 99.4B | $n=23$ |
| $KCl,PtCl_2$ | | | | | | | Pt | | Platinum. | | PtO | | | $PtO_2$ | | $OsO_4$ | 98 | 98.5B | $n=23$ |
| | | | | HgSe | | | Hg | | Mercury. | $Hg_2O$ | HgO | | | | | | 100 | 100 | $n=24$ |
| | | | | PbSe | PbS | | Pb | | Lead. | $Pb_2O$ | PbO | | | $PbO_2$ | | | 104 | 103.7 | $n=25$ |
| | | | | AgSe | AgS | | Ag | | Silver. | $Ag_2O$ | AgO | | | | | | 108 | 108.1 | $n=26$ |
| | | | | | | | Au | | Gold. | | AuO | | | | $AuO_3$ | | 198 | 197 | $n=43$ |

## $4+n4$ or $2+n4$.

| ISOMORPHS. | | |

$1 + n3$.

| HOMOLOGUES. | | | | | | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | Theo. | Observed. | n |
| GlO | | | | | | | 4 | 4.7 | $n=0$ |
| MgO | | | | | | | 12 | 12 | $n=2$ |
| | $Al_2O_3$ | | | | | | 14 | 13.7 | $n=3$ |
| CaO | | | $CaO_2$ | | | | 20 | 20 | $n=4$ |
| TiO | | | $TiO_2$ | | | | 26 | 25.2B | $n=6$ |
| CrO | $Cr_2O_3$ | $Cr_3O_4$ | $CrO_2$ | $CrO_3$ | $Cr_2O_7$ | | 26 | 26.7 | $n=6$ |
| MnO | $Mn_2O_3$ | $Mn_3O_4$ | $MnO_2$ | $MnO_3$ | $Mn_2O_7$ | | 23 | 27.6 | $n=6$ |
| FeO | $Fe_2O_3$ | $Fe_3O_4$ | $FeO_2$ | | | | 28 | 28 | $n=6$ |
| CoO | $Co_2O_3$ | $Co_3O_4$ | | | | | 30 | 29.5R | $n=7$ |
| NiO | $Ni_2O_3$ | | | | | | 30 | 29.5R | $n=7$ |
| CuO | | | | | | | 32 | 31.6B | $n=7$ |
| ZnO | | | | | | | 32 | 32.5Erd. | $n=7$ |
| | $Zr_2O_3$ | | | | | | 34 | 33.6B | $n=8$ |
| SrO | | | $SrO_2$ | | | | 44 | 43.8 | $n=10$ |
| LaO | | | $LaO_2$ | | | | 48 | 47 | $n=11$ |
| CeO | $Ce_2O_3$ | | | | | | 48 | 47.3M | $n=11$ |
| RO | $R_2O_3$ | $R_3O_4$ | | | | | 52 | 52.1B | $n=12$ |
| RuO | $Ru_2O_3$ | | $RuO_2$ | $RuO_3$? | | | 52 | 52.1B | $n=12$ |
| PdO | | | $PdO_2$ | | | | 54 | 53.3 | $n=13$ |
| CdO | | | | | | | 56 | 56 | $n=13$ |
| SnO | $Sn_2O_3$ | | $SnO_2$ | | | | 58 | 58 | $n=14$ |
| ThO | | | | | | | 60 | 59.6 | $n=14$ |
| UrO | $Ur_2O_3$ | $Ur_3O_4$ | | | | | 60 | 60 | $n=14$ |
| BaO | | | $BaO_2$ | | | | 68 | 68.5 | $n=16$ |
| IrO | $Ir_2O_3$ | | $IrO_2$ | $IrO_3$ | | | 98 | 98.5B | $n=24$ |
| OsO | $Os_2O_3$? | | $OsO_2$ | $OsO_3$ | | $OsO_4$ | 98 | 99.4B | $n=24$ |
| PtO | | | $PtO_2$ | | | | 98 | 98.5B | $n=24$ |
| HgO | | | | | | | 100 | 100 | $n=24$ |
| PbO | | | $PbO_2$ | | | | 104 | 103.7 | $n=25$ |
| AgO | | | | | | | 108 | 108.1 | $n=26$ |
| AuO | | | | $AuO_3$ | | | 198 | 197 | $n=49$ |

| ISOMORPHS. | | HOMOLOGUES. | | ATOMIC WEIGHTS. | | |
|---|---|---|---|---|---|---|
| 3 | 1 | | | Theo. | Observed. | n |
| | | Hydrogen. | $H_2O$ | 1 | 1 | $n=0$ |
| | LiCl | Lithium. | LiO | 13 | 13 | $n=4$ |
| $NaO,SO_3$ | NaCl | Sodium. | NaO | 46 | 46 | $n=15$ |
| | KCl | Potassium. | KO | 79 | 78.4 | $n=26$ |

AFFILIATIONS.

| | | | | | | |
|---|---|---|---|---|---|---|
| $AgO,SO_3$ | | Silver. | AgO | 217 | 216.2 | $n=32$ |

EXPLANATION OF THE TABLE.

The formula at the head of each series is a general expression for the atomic weights of that series. The names of the series are derived from the "Common Differences," which are the numbers multiplied by $n$ in the general formulæ. In the columns headed "Theoretical" are given the atomic weights calculated from these formulæ and the values of $n$ given in the last columns at the right of each division of the table. In the columns headed "Observed" will be found the observed values of the same atomic weights. These have been taken from the table of atomic weights given in the last volume of Liebig and Kopp's *Jahresbericht* (for 1852), with the exception of those against which are placed the initials of the observers. The last were taken from Weber's *Atomgewichts Tabellen*. In some cases the atomic weight is taken at twice its received value, but it is then underlined. The compounds in any one column at the right of the names of the elements are homologous. In the same way, those in any one at the left are isomorphous. The numbers at the head of these last columns indicate crystalline systems as follows: 1. Monometric; 2. Dimetric; 3. Trimetric; 4. Monoclinic; 5. Triclinic; 6. Hexagonal. The data from which the table was compiled were drawn from numerous sources, but especially from the following works: Gmelin's *Handbook of Chemistry*, Graham's *Elements of Chemistry*, Phillips's *Mineralogy* by Brooke and Miller, and Gustav Rose's *Krystallo-chemische Mineralsystem*. References have been given only in a few cases, to avoid crowding the tables. For authorities in other cases, the author would refer to the abovementioned works.

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## Correspondence.

*Extract from a letter from Dr. W. I. Burnett to Prof. J. D. Dana.*

Magnolia, Florida, March 1, 1854.

* * * * * * *

Florida still remains almost a *terra incognita* to naturalists, although in geological structure, and Fauna and Flora, one of the most interesting States in the Union. Even from the limited survey I have made of its surface and general features, I am persuaded that some erroneous views have been entertained as to its formation and the exact nature of its peninsular relations. Naturalist after naturalist, from the time of the quaint old Bertram to the present day, have traversed its various sections, collecting specimens relating to their particular line of study, but no report has yet been made, in full, of the

geological structure, the agricultural resources, or the Fauna and Flora of the state.*

Gen. Bernard's report of a survey by a corps of topographical engineers for a practical object, contains the greatest number of details, as well as the most reliable, upon the general features of Florida, that I have seen. In making these notes I shall draw somewhat from this report, especially as to measurements. From the southern boundaries of Georgia to a line drawn from Cape Canaveral to Tampa the land is covered with pines, is much undulated, and in some places even hilly. The waters that run into the Atlantic Ocean and those that run into the Gulf of Mexico are divided by a ridge which extends slopingly from north to south, terminating at the above-mentioned line. The height of this ridge is certainly much greater than I had supposed, or than is generally understood even by those who have traversed the state, for the land rises very gradually and, without instruments, one is wholly deceived. The highest elevation is situated one mile west of Kinsley's pond and seven miles east of Sampson's pond, and is two hundred and thirty seven and one-half ($237\frac{1}{2}$) feet above low tide in the Atlantic Ocean. There are other points on this ridge between 150 and 200 feet high; and, distant only a few miles west of St. John's river, I have traversed elevations, at least one hundred feet above the stream that encircled their base and ran towards the sea. I am particular thus to allude to this point in order to correct the erroneous opinion entertained by many that the whole peninsula of Florida is a flat sandy country, and not elevated more than fifteen feet any where above the level of the sea. The soil is sandy, but the hummocks contain red, yellow, or black clay, mixed with sand. The upper stratum is about six feet of sand, and the substratum appears to be limestone mixed with much sand. Scattered over the surface are more or less conical depressions or *sinks*. Those that I have seen are of pretty regular form, though of variable size; they contain no water, and are probably due to the substratum of limestone crumbling or being undermined from some cause. Nowhere so far as I am aware, do these depressions appear to be fed by intermittent springs, like those of the pine region of South Carolina, which have already been described by Mr. Tuomey.

In this connection, I may mention that Bernard's levelling parties commenced, one division at the Gulf of Mexico, the other at the Atlantic Ocean, and, proceeding inwards, met at Alapaka. At this point it was found that they were at an elevation of ninety-five feet above the level of low water of the Atlantic, and ninety-one and a half feet above the level of the low water of the Gulf—making three and a half feet difference between the low water level of these two waters. The same result was obtained by a repetition of this same levelling. The fact is quite interesting in connection with the phenomena of the Gulf Stream.

* Buckingham Smith, Esq., is now engaged upon a detailed history of Florida, as I understand. The great pains he has taken to consult ancient documents relative to its primitive settlement, as well as his personal knowledge of the state physically, will ensure from him a most valuable work. But it is hoped that the legislature of this state will soon take measures for having a careful physical survey of Florida, by a competent geologist and naturalist.

But the physical features of the southern extremity of Florida are, as is well known, the most interesting. It is still land incompletely formed, and the conditions of its successive growth are yet visible. It is needless for me here to describe these conditions or to enter upon the details of this coral built promontory. Within a few years Agassiz has, as is well known, carefully studied not only the phases of the formation of the Keys, but also the coral-polyp marvels of the deep, which have led to these almost continental changes. Between the ridge above referred to and the everglades, is that remarkable collection of fresh water known as *Lake Okechobee.* Its average diameter is about thirty miles, and it is reputed to be quite deep. It contains a few islands, and is the reservoir of the neighboring streams, and of the Kissime river. South of this lake are the everglades which lie in a vast basin of limestone. They consist of a shallow lagoon—their waters varying from one to six feet, studded with islets, trees, and a rank growth of vegetation. Their waters are fresh and due to the copious autumnal rains, and as the rim of the basin is some twelve or fifteen feet above the ocean, the slow moving currents set southwards towards the sea. This section of Florida, as shown by Agassiz in his recent lectures on the Florida reefs, is of coral origin and of comparatively modern date. Thus recently formed, the geology of this promontory may be easily inferred. It is oolitic limestone filled with the shells and corals, and the fossil remains of species that still exist. The present state of the Indian affairs did not allow me to visit this southerly point, but from intelligent gentlemen connected with the army, I learned that, besides the curious geological features I have above alluded to, this land presents many wonders and objects most interesting to the naturalist who will have the intrepidity and hardiness to explore it.

High up on the St. John's river there are from point to point vast deposits of fluviatile shells. At Enterprise especially, these form even bluff-like elevations of forty to fifty feet, and of considerable extent. Hereabouts have been found also the fossil remains of Vertebrata that still exist. The whole physical structure of this region is of great geological interest, and would well repay the most careful examination.*

One of the most interesting features of this State is the St. John's River. It furnishes a key to the whole plan of formation of a large portion of Florida. As is well known, its course is northward, unlike that of other American rivers, and this fact alone would indicate that the conditions of its primitive formation are different from those of other streams. It is in fact a chain of lakes, as its Indian name signifies, (*Welaka,*) and it rises in the Cypress Swamp, situated near Indian River. Its similarity in appearance to an arm of the sea, is indeed more than apparent, for in its course of 300 miles, with often a width of six miles, there is a fall of only about twelve feet, and the tide is felt 100 miles above its mouth. But fed mostly by lateral infiltration chiefly on

* Our friend, Prof. Bailey, has been all about the travelled sections of Florida with his microscope, and his recently published results show that he had his eyes constantly and attentively upon the "invisible forms of life." He has also noticed, these grosser features (p. 23) which certainly are anything but microscopic. See Microscopical Observations made in South Carolina, Georgia and Florida, by J. W. Bailey, &c., &c. Smithsonian Contributions to Knowledge, vol. ii, Art. 8.

its western side, and receiving, besides, a few large tributaries, this majestic river, slow moving as it must be, discharges an enormous quantity of water into the sea. The segment of land included between its mouth and a line draw from its source to the sea, is at no point elevated above 12 or 14 feet, that is, higher than the fall of the river itself. It is therefore probable that this segment, like the whole southern extremity of the state, is of comparatively recent origin previous to which, of course, the St. John's River did not exist. This stream, consequently, is not properly a river, but rather an arm of the sea shut off from the ocean by alluvial sandy deposits. It receives the waters which arise on the easterly side of the ridge of the state, precisely as though it were the sea itself. Indian River presents the same phenomena, excepting that it is not wholly shut off from the ocean, but is open by inlets; and were it not for the influence of the Gulf Stream, the reef which forms its ocean boundary would be indefinitely extended until this river became truly an inland stream. By this view of the gradual geological changes that have supervened in this state, it would appear that ancient, primitive Florida, must have been of quite insignificant size,—indeed, limited to the grand dividing ridge which perhaps is a spur of the upheavals of the states that bound it on the north—a narrow promontory thus extending southward from the main land, and only about half the length of the present peninsula. But the conditions of the formation of what may well be called modern Florida, have an additional interest from their relations to the phenomena of the Gulf Stream. Agassiz has shown that the coral building is now at an end on the southern extremity of this promontory, since the depth of the Gulf Stream as it shoots round this point is incompatible with the conditions of life of the coral polyp; and so all hope of an annexation, physical at least, of this continent to Cuba, is in vain. It is a curious question, What were the phenomena of this stream when ancient Florida alone existed? Is it possible that with a current anything like the present, the slow-growing coral-formation could have made progress to the extent we well know it really has?

The Fauna and Flora of Florida seem much like those of Georgia and Alabama, as far as they go, for I have been surprised to find so great a deficiency in a land having so genial a climate. But this deficiency does not exist in one class—that of reptiles—at least, in the Saurians and Ophidians. The herpetologist will here find both venomous and non-venomous snakes which will meet his desires both as to numbers and size; and that prince of American reptiles, the alligator, is here seen in an abundance that is almost incredible to any but the beholder. The natural history, the anatomy and physiology of this kingly Saurian has yet to be written, and a pleasant as well as a most honorable task remains for some American anatomist to monograph this subject, which he could easily do by placing himself for a season on some convenient spot on the St. John's river. Aside from the special anatomy of the animal, I am persuaded that careful, well-conducted physiological experiments, especially upon its nervous system, would furnish results of great value in physiology. Here the whole relations of the so-called reflex actions of the nervous system are presented upon a most magnificent scale.

The experiments that I have already made have proved deeply interesting to me, and have modified somewhat the opinions I have hitherto entertained upon this class of nervous actions. But this point will have my special consideration at another time.

I cannot close this letter without alluding to a subject which, among others has engaged my attention during my sojourn in Florida this winter for my health. I refer here to the orange insect or *Coccus*, which has almost entirely ruined the prospects of an occupation, one of the most delightful, as also one of the most profitable that man could here engage in—the culture of the orange. The climate and soil, especially of east and south Florida, is excellently fitted for the growth, almost to perfection, of this most delicious fruit. All along the St. John's river there are groves of the wild-orange that salute the eye most pleasantly, although this fruit is too acid to be palatable except when used to make a beverage (orangeade).

The China orange (*Aurantium sinense*) is the species which has here been introduced for cultivation as a commodity. Here it grows most luxuriantly, and, for size and flavor, has always had the preference in the market, even to those produced in more tropical regions. Some fifteen years since when the orange-culture of this state was in its most favorable condition, the revenue from this business was indeed wonderful: 6,500 oranges have been yielded by a single tree, 2,000 being a not uncommon yield. At a small place called Mandarin, situated on the St. Johns river, thirty-five miles from its mouth, there were annually produced 1,500,000 oranges. At other places, such as St. Augustine, there was a yield of even a greater number.

The cold of the winter of 1835 seriously affected the vitality and prosperity of the trees throughout the peninsula. But the great drawback now to their successful culture, is undoubtedly, the orange insect or *Coccus*. This insect appeared so as to be felt about the year 1838, and its increase and distribution since has worked most lamentable effects. Section after section has yielded to its ravages, and those fine groves above mentioned have become nearly ruined. The trees seem blasted, and only yield a very small quantity of fruit. Some of the groves I visited are now completely deserted; all hopes of an extermination of the pest being relinquished. Some trees that I examined seemed completely covered, trunk, limbs, leaves, and fruit, with this insect, and it is not at all strange that the vitality of the trees yields to such a draft upon its juices.

Careful, from scientific reasons, not to lose so excellent an opportunity for the study of the animal, and desirous, from practical reasons, to find out, if possible, a means for its extermination,—this insect has received no little attention from me. It is exceedingly small and insignificant for so disastrous a worker, the males being only $\frac{1}{30}$ and the females about $\frac{1}{20}$ of an inch in length. On this account, its anatomy and physiology could be successfully studied only by the microscope; but the results afforded have well repaid the labor thus bestowed. These results are certainly sufficiently important for a special consideration, which I propose to give them, at some future time. It will suffice for me to say here, that the conditions of their reproduction are remarkable, and quite favorable for the prodigious multiplicity of this

animal. The life of the females is almost vegetative; with an outward form so undeveloped as to quite resemble a larva, she fastens herself to the leaf or bark, and, drawing the juices therefrom, develops her ovaries, successive litters of eggs being formed; and, as the conditions of her life would be wholly incompatible with the formation of eggs and their deposit, as in most other insects, these eggs undergo most of their development in the mother,—escaping from her just before they hatch. The orange Coccus is therefore viviparous. The male appears only at certain seasons, and is literally only a locomotive male apparatus, for his digestive organs are undeveloped, and as the testes, &c. are already fully formed at his first appearance, he lives only to impregnate the females. The means I shall recommend for a removal of this pest, will be based upon a consideration of the intimate economy of the animal.

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*Correspondence of M. Jerome Nicklès, dated Paris, February 27, 1854.*

At one of its late sessions, the Academy of Sciences awarded the annual prizes to authors of works on different subjects proposed by it, and also "encouragements" to those who have distinguished themselves by researches in their own proper domain. As the list of prizes is long, we will only mention those of more special interest.

1. The Academy awarded the *prize in Mechanics* to Felix Franchot (1) for the invention of the lamp known under the name of "lampe à moderateur," brought forward by him in 1836 and 1837, the use of which has now become general; (2) for his experiments in the construction of motive engines with hot air, which he has pursued with unceasing perseverance since the year 1836. We have already spoken of the researches of M. Franchot and will not return to them.* The report upon them was made by the distinguished engineer and mechanician, M. Combes, at this moment President of the Academy of Sciences.

2. Prize Extraordinary on the Application of Steam to Navigation. In November, 1834, the government established a prize of 6000 francs for the work or memoir which should occasion the greatest advance in the application of steam to navigation and to naval force. Since this epoch, until 1848, great improvements have taken place, but they were not first put in practice in France; they were brought out in America and England. But now, by the construction and success of the "Napoleon," a vessel of the line with sails and steam, the end of the prize has been completely attained. The Academy has consequently assigned the prize (1) to M. Dupuy de Lôme, officer of the naval Engineer corps, for devising and constructing the ship Napoleon with sails and steam having a screw propeller, which unites in a remarkable degree rapid sailing and excellent sea qualities. (2) A prize to M. Moll, officer of the Naval Engineer Corps, sub-director of naval construction at the great manufacturing establishment of Indret, for having calculated the theory of, and constructed, the mechanism of the Napoleon, and for having, in

* In awarding the prize to M. Franchot, the Academy does not touch the question of priority as to hot-air engines, it being well known that M. Franchot in this respect has others before him.

connection with M. Bourgois, made the experiments with the screw propeller, the results of which are now the rule with engineers. (3.) To M. Bourgois, Captain in the Navy ("de fregate") for his labors on the screw propeller, and for his views on the progressive transformation of the existing naval marine into a mixed marine of sails and steam. The Report made by M. Charles Dupin, is extremely interesting, as respects the history of steam navigation, and we regret that we cannot find place for it here.

The question proposed in 1852 for the physical sciences was the following: "*To discover by direct observations and experiment the mode of development of intestinal worms and that of their transmission from one animal to another, and to apply the anatomical and physiological facts thus brought out to determining their affinities.*" In pursuance of the remarkable Report of M. de Quatrefages, the Academy awarded the prize to M. van Beneden, Professor at the University of Louvain (Belgium), and made honorable mention of M. Küchenmeister of Zittau (Saxony).

The prize in Experimental Physiology has been awarded to M. Claude Bernard for his discovery, with regard to the influence, which the cervical portion of the Great Sympathetic Nerve exerts on the temperature of the parts, to which its threads are distributed, accompanying the arterial vessels.

Another prize has been given for the *Traité de Chimie Anatomique et Physiologique* of MM. Robin and Verdeil, on account of the new facts which it contains and the thorough manner in which chemical facts in their relation to medicine are presented.

Among the prizes, there are also, the medals awarded to MM. de Gasparis, Chacornac, Luther and Hind for the discovery of five new planets; a prize to M. Fontaine and M. Machecourt for a parachute for the use of miners; an "encouragement" to M. Chuard for his attempts towards perfecting the miner's safety lamp. Other prizes for works on Statistics, Medicine, Surgery, and a grand prize to M. Hervé Mangon for his "Etudes sur le drainage au point de vue pratique et administratif."

Among the subjects of prizes offered we mention only those pertaining to the physical sciences.

1. For 1856.—A rigorous and methodical investigation into the metamorphoses and reproduction of the Infusoria properly so called, (the Polygastrica of M. Ehrenberg).

2. For 1855.—An exposition of the laws governing the distribution of fossils in the different sedimentary strata in their order of superposition; and a discussion of the question of their appearance or disappearance, successive or simultaneous.

A research into the nature of the relations existing between the present and past states of the organic kingdom.

Another for 1856.—The determination, through the study of the development of the embryo in two species, one taken from the class of Vertebrata, and the other either from the Mollusca or Articulata, of the proper foundation for comparative embryology.

The prizes for either of the above is a gold medal of 3000 francs value.

A medal of gold, of 800 francs, is decreed each year to the work, printed or in manuscript, which appears to have contributed the most to *the Progress of Experimental Physiology.*

A gold medal of the value of 2500 francs, is offered for 1856, for the best work on the *mode of fecundation* of eggs, and the structure of the organs of generation, in the principal natural groups of the class of polyps, or of that of Acalephs.

A large number of prizes are offered in mathematics, medicine and physiology, and NOTHING for physics, chemistry or mineralogy; it would seem as if these sciences had no existence. It is true that the Academy of Sciences of Paris has little to show among the great discoveries which physics and chemistry have accomplished in these later years; certainly such discoveries have not been called forth by any action on the part of the Academy.

*Deaths and Academic Elections.*—The Botanical Section has lost this year three of its members: A. DE JUSSIEU, President of the Academy, AUGUSTE DE ST. HILAIRE, and more lately GAUDICHAUD; and in the preceding year it lost RICHARD, Professor in the Faculty of Medicine. It has been recently occupied with filling these vacancies. MONTAGNE and TULASNE replace MM. Richard and de Jussieu; and A. de St. Hilaire is succeeded by M. MOQUIN-TANDON, the celebrated author on Vegetable Teratology, at present Professor in the Faculty of Medicine.

*Zoological Society for the Acclimation of Animals.*—Since we are in the midst of Natural History, we hasten to announce the formation of a Society whose end is to apply the Science of Zoology to the Acclimation and Domestication of valuable animals from different regions of the globe. Its founder and President is M. Isidore Geoffroy Saint Hilaire, son of the illustrious zoologist, and who should not be confounded with the botanist, M. Auguste de St. Hilaire, to whom we have above alluded.

M. Is. G. St. Hilaire is Professor of Zoology at the Jardin des Plantes, and for a long time he has prepared himself for the noble work which, aided by several other savants and the principal great proprietors of France, he is about to undertake. The end of the Society is to promote (1) the introduction, acclimation and domestication of species of animals either useful or ornamental. (2.) The perfection and multiplication of new varieties, introduced or domesticated. The number of members of the Society is not limited, and foreigners may unite in it. And among the latter, there are already Prince Demidoff, MM. Graëlls, Ramon de la Sagra, Vilanova of Madrid, and a large number of Belgians, Swiss and Germans, and some from the United States.

*Artificial Production of Pleochroism in Crystallized Substances.*—The influence which small quantities of a foreign substance chemically inert, exert upon the physical properties of bodies, as their density, index of refraction and angle of polarization, has long been known. Some years since,* I showed that these causes act also on the angles of crystals, and at times produce modifications as great as a change of the type and system of crystallization, and thus may give rise to dimorphism.† Since then M. Hugard has observed facts of a similar

* Comptes Rendus de l'Acad. des Sci., 1848.

† Comptes Rendus de Laurent, etc., 1850, and Annalen de Ch. et de Phys., 1853.

kind in sulphate of strontian, and M. G. Rose, in tetradymite; according to the latter, foreign matters cause the rhombohedron of tetradymite to vary nearly 3 degrees from that of the pure metals.

In view of such facts, my opinion appears less exaggerated than at first. The two forms of dimorphous bodies are in general approximate forms, whose prisms, although pertaining to distinct systems, differ between them only a few degrees in angle. I have inferred that a foreign substance present, may at times modify the molecular force which presides in crystallization, so as to give the molecules what is needed to cause them to pass from one system to another.

Should a new planet suddenly appear in our system, all the other planets would feel it as much as if a planet were withdrawn. A crystal is in my view a planetary system formed of atomic planets in motion; foreign molecules or planets abruptly introduced, bring in their own attraction and proper movements, and impress on the other atoms another condition of equilibrium.

Thus it may be with foreign matter which is chemically inert, as in my observation on the bimalate of ammonia, in which the foreign substance was the coloring material that was eliminated entirely by some crystallizations in pure water. If on the contrary the substance is active chemically, the form may change either totally or partially. These last are what I have called examples of *hemimorphism*, which I have illustrated by glycocoll and its combinations, oxalate and chlorhydrate of methylammine, etc.

It is not my intention at this time to touch on these questions. They have been called up by the reading of an important paper by M. Senarmont on the artificial production of pleochroism;—a property which he communicates to certain salts by introducing into them through crystallization, foreign substances that will not unite with them chemically, and which may be eliminated spontaneously by some solutions followed by crystallization in distilled water. He has thus communicated pleochroism to the nitrate of strontia, by crystallizing it in water colored with log-wood, changed to purple by some drops of ammonia.

*Derivatives of nitrotartaric acid.*—M. Dessaignes, who devotes his leisure hours to science, has brought out several new facts of interest. He has observed that the nitrotartaric acid which he has obtained, decomposes spontaneously in water, producing an acid which he calls *tartronic acid*, $C_6H_4O_{10}$. Heated to 160° C., it loses carbonic acid and leaves a residue which appears to correspond to $C_4H_2O_4$; this in contact with potassa affords a salt, the acid of which has the composition $C_4H_4O_6$ and is identical with glycollic acid, extracted from the sugar of gelatine. This acid forms an *amid* which is not the sugar of gelatine although like it in composition. Glycollamid is an isomere of glycocoll, just as lactamid is an isomere of alanine. M. Dessaignes considers the insoluble substance $C_4H_2O_4$ as having the same relation to glycollic acid as lactid to lactic acid; and he hence calls it glycollid.

*On the gluten of wheat.*—M. Millon, compelled by his high military position to rather a nomadic life, has for some years suspended the fine researches which he had undertaken—researches on the oxydized compounds of nitrogen, chlorine, mercury, nitric ether, also on vegetable physiology, etc., which had given him a high rank among men of science. Removed from his laboratory and sent to Africa, for political

reasons, he has found the means of carrying on some important investigations without a chemical laboratory, and he has just now brought before the Academy a series of papers which he proposes to present, containing the results of some researches on wheat.

In his first memoir, he brings out the important fact that there are some kinds of wheat, of good appearance, *that contain no gluten*. His attention was called to the subject by the wheat of Guyotville (Algeria), which although appearing well, was nearly destitute of this important ingredient. He was thus led to examine a quantity of the wheat poor in gluten, and he found it to be a mixture of rich grains with others containing none of this albuminoid substance. Dough made from the wheat of Guyotville without gluten is worked with more difficulty than ordinary dough, and the bread is swallowed with some difficulty, like that which is dry or stale. The nitrogenized substance of this wheat is soluble in water.

In a second memoir, M. Millon takes up the chemical composition of different varieties of wheat, and he deduces from his results a distribution of the wheats—using terms already in use—into *tender wheat*, and *hard wheat*, the characters of which are as follows:

*Tender wheat:* Fracture white, opaque, and farinaceous, the starch escaping more or less abundantly; a more or less complete replacement of the gluten by a soluble albuminoid principle varying widely in the proportion of nitrogen.

*Hard wheat:* Fracture horny, semi-translucent, without a starch-like appearance; all the nitrogen existing under the form of gluten and the weight of it always a little superior to the quantity of albuminoid principal represented by the nitrogen; only small variations in the proportion of nitrogen, the amount of which is large. This last characteristic does not serve to distinguish the hard wheat, since it is not rare to meet with tender wheat containing as much nitrogen as the hard wheat, or even more.

Wheat intermediate between these two varieties, M. Millon names *semi-hard* wheat, which he describes as follows:

Fracture close and less horny than in hard wheat; whitish when crushed; a proportion of gluten mixed with the albuminoid principle; a large proportion of nitrogen, and this nearly constant.

These descriptions are completed by a mention of the external characters, taken from the volume, color, integuments, etc. His facts are derived mainly from the wheat of Algeria and those of the north of France, and it remains to make the results general, and applicable to wheat of whatever origin.

*Natural History of Lupulin.*—This proximate principle of Hops, was first examined by Dr. A. W. Ives, of New York; MM. Raspail, Payen, Chevallier and others, have since been occupied with it, without fully establishing its chemical constitution. M. Personne, Assistant at the School of Pharmacy at Paris, has taken up the subject, and given it a satisfactory solution.

He has found that when acted upon by boiling water, lupulin affords two groups of substances; one obtained by distillation with water and the other by means of steam.

The former consist of *valeric acid*, and of *valeral* or *valeric-aldehyde*. The matters which are volatilized only through the action of steam,

consist of an organic and a nitrogenized substance, which the author has now under examination.

*Various Communications.*—In the name of a Commission consisting of MM. Thenard, Balard and General Piobert, M. Balard read a very favorable Report on the researches of M. Violette on the carbonization of wood by means of over-heated steam, of which we gave an account in the number for September, 1853.—M. Payen read a memoir, demonstrating the presence of carbonate of lime in vegetables.—A naval officer, M. Tramblay, read a paper on a new apparatus for saving property at sea.--M. Thomas, of Colmar, presented an arithmometer upon which he had worked for thirty years, which, as appears, resolves completely the problem of calculating machines: we shall return again to it.—At each session of the Academy, communications flow in from the four quarters of the globe both on the subject of the Bréant legacy (100,000 francs, to the person who shall discover the cause and cure of cholera), and that of the prize proposed respecting the disease of the vine. The largest number of communications come from Germany. This earnestness is no doubt praiseworthy, and yet may excite a smile; for each of the authors pretends to have resolved the problem better than all others, and the number of panaceas discovered or announced now amounts to thousands.

In the year X. of the first Republic, the French Government founded a prize of 60,000 francs, to be given to the person who should through experiment and discovery, make a step in electrical science comparable with that of Franklin and Volta, to be adjudged by the Academy of Sciences. The fact of this offer has recently been called to mind by Madame Œrsted, who claims the prize in the name of her late husband, M. Œrsted. A commission consisting of MM. Pouillet, Becquerel, Despretz, Thenard and Regnault, have been charged with the examination of this demand. They find much embarrassment, since without contesting the merit of Œrsted's discovery, his is not the only important one made since the year 1801. The discoveries of Davy, Ampère, Arago, Faraday, Ohm, Morse, Wheatstone, Jacobi, the pile with a constant current discovered by M. Becquerel, the thermo-electric currents discovered by Seebeck,—all show that it would have been far easier to have awarded the prize in 1820 than at the present time.

*Industrial Photometry.*—An instrument by Mr. Babinet for exact photometric measurements, has for a long time been used, which is based on the neutralization of the tints of polarized light, as employed in photometry by Arago. We have not space for a figure and complete description of this small apparatus, which the ingenious Duboscq has rendered very portable and convenient for use. It consists essentially of a tube having a ground glass at one end, and at the other end an analysing prism of Iceland spar. A pile of plates of glass, serving as a polarizer is fixed to the tube so as to form with it an angle of 35°, the angle of polarization of the glass. The light diffused upon the ground glass, reaches the eye only after having passed across the pile of glass, and is consequently polarized by refraction perpendicularly to the plane of incidence of the rays.

The ground glass is for receiving the illuminations for comparison. It is successively illuminated by the lights to be compared. On traversing the pile of glass plates, the rays are brought into a condition to

color the four semicircles of a Soleil's polariscope, which is furnished with plates, having the property of double rotation. By means of a third source of light, a second ground glass is illuminated, which is placed obliquely so as to neutralize the colors produced by one of the luminous sources under comparison; then leaving this light fixed with relation to the instrument, the photometer is moved towards or from the second source of light, until the colors disappear anew. The relation of the square of the distances give, then, the relation between the intensities of the two lights. Different precautions are necessary when it is not convenient to vary the distance of the photometer from each of the sources of light, as, when they are two jets of gas, or of electric light, not easy of access. But it is impossible here to enter into these details.

*On forming vessels of gold by the aid of phosphorus.*—The property of phosphorus, of precipitating certain metals from their solution has long been known; and gold is among the number. M. Levol has used this process in forming gold vessels useful in chemical research. He takes the perchlorid of gold, and places in it, at the ordinary temperature, some phosphorus, moulded of a form convenient to serve as a nucleus for the vessel of gold. To give the phosphorus the desired shape, it is melted in a water-bath near 60° C. in temperature, within a vessel of glass having the form required. After cooling it, the phosphorus is taken out solid, from its envelop, breaking it, if it be necessary. The precipitation of the gold or the construction of the vessel is then begun; and it finally remains only to remove the phosphorus by re-melting it and washing by the aid of boiling nitric acid until the last traces are removed.

*Gilding of Silk, &c.*—Commerce has furnished for some time a kind of silk treated by a galvanoplastic method; the threads produce a costly fabric of wonderful solidity. The author of the process is M. Pouilly. He first metallises the silk, then covers it with a thin layer of copper and finally applies the gold by the aid of the galvanic battery.

Similar trials have been made within a few years by Madame Foa. The gold is dissolved in aqua regia, then dried by evaporation, then it is dissolved again in water, which is diluted with a large quantity of distilled water. The fabric under treatment was placed in this bath; the salt of gold fixes itself upon the ligneous fabric, and the compound is reduced by an exposure of the tissue to the action of hydrogen. This process has been abandoned, as it proved to be not uniformly successful, though without reason, as success once is enough to insure constant success after the causes of failure are studied out.

*Local anesthesis.*—The process invented by Mr. Harris enables the surgeon to render insensible the part of the body to be operated on, without affecting the rest of it. MM. Nélaton and Paul Dubois, two skillful surgeons of Paris have made with the apparatus the following experiments. After having for five minutes directed a jet of the vapor of chloroform on an abscess, Dr. Nélaton was enabled to make an incision into the hollow of the foot without the least sign of pain on the part of the person operated on. The following fact observed by Dr. Dubois is still more remarkable. A young woman had an abscess in the armpit and an ulcer on the wrist. She suffered much from both and could not use her arm. The first application of chloroform was made on the armpit. The tumor, although so painful that she could

not bear to have it touched, became so completely insensible, that it could be handled without pain, and the woman could raise her arm; the insensibility continued for three hours. The operation was not performed that day. When the abscess was at its head, the chloroformic fumigation was renewed, and almost immediately Dr. Dubois plunged the knife into the abscess without any sensation on the part of the patient; he then turned her attention from it, and after this there was no more feeling of pain in the region. Afterwards the small place on the hand, fumigated in the same way, also remained completely insensible.

These facts are strongly affirmed by two honorable physicians who also cite the witnesses to the operations. Other doctors have attempted to repeat the experiment; but, as is singular, *without success.* More study of the conditions necessary may lead to new and complete success; and we hope to announce such in our next communication.

*Influence of bismuth on the ductility of Copper.*—There has recently arrived from Australia a black copper in ingots possessing some peculiar properties. Although of high per-centage the color is bronze; it is but little ductile; the fracture is loose and crystalline, which may be removed by refining according to the ordinary methods.

M. Levol, Assayer at the Mint of Paris, has analyzed this copper before and after refining, with the following results:

| | Crude. | Refined. |
|---|---|---|
| Copper, | 99·400000 | 99·480000 |
| Sulphur, | 0·314000 | |
| Lead, | | 0·362000 |
| Silver, | 0·100000 | 0·100000 |
| Bismuth, | 0·144000 | 0·048000 |
| Gold, | 0·000833 | 0·000833 |
| Tin, Antimony, | *trace* | *trace* |
| Arsenic, | | |
| Loss, | 0·041167 | 0·008917 |

The lead and trace of arsenic proceeded from the process of refining; and it is found by experiment that the small proportion of antimony, arsenic, gold, silver and lead do not explain the want of ductility of the copper. The bismuth, then, only one-third of which had resisted oxydation, is the sole cause of the loss of ductility. M. Levol has proved the correctness of this conclusion by preparing different alloys. It is remarkable that bismuth which has so many points of resemblance to lead, should be so different in the above respect. It is important to examine for bismuth the coppers of commerce, in order to search out the cause of the peculiar mechanical and chemical qualities often found, even in copper of excellent appearance.

*On the bronze employed in sheathing ships.*—This subject has been alluded to in the November number of this Journal. Some new facts have been brought out by M. Bobbierre, confirming the results before announced; and he has established them by experiments. For the purpose of experiment, he has made with metals either pure or impure, ingots of bronze of a cylindrical form by castings in sand, having a height of 40 centimeters and weighing 25 kilograms. Portions for analysis were taken from different parts of the ingots, both from the

surface and interior. The central parts in all cases contained less tin than the surface. For example, in the alloy of 97 of copper and 3 of tin, the richness in tin for the two parts had the ratio of 1 to 3·97. On adding to the alloy 1 p. c. of zinc, the homogeneity was much increased, the ratio becoming 1 : 1·45.

Under Louis XIV, the cannon were of better quality than those of the present time; zinc was mixed with the metal, in the condition of *brass*. The trials made in our time have failed because the zinc was introduced directly into the alloy while in fusion, in which case the zinc is burnt off and forms no combination with the fused metal.

*Traité des Poisons, ou Toxicologie appliquée à la Medicine legale, à la Physiologie et à la Therapeutique*, par le Docteur CH. FLANDIN. 3 vols. in 8vo. Paris, chez Mallet-Bachelier.—The first volume of this work has been some time out and has been favorably received. The author does not admit of poisons in the sense ordinarily understood. Poisonous substances are not, as has been said, irritants, etc., but substances not capable of being assimilated, which penetrate into the organism by absorption and become an obstacle to chemical or physiological action on which life depends. Thus the pathological and therapeutic department of poisoning are distinct. These views characterize the work. All the processes for detecting poisons before and after inhumation are given with details, among which are some that have given a just reputation to M. Flandin, their discoverer.

*Traité de l'Electricité théorique et appliquée;* par AUG. DE LA RIVE. 2 vols. in 8vo. Paris. Baillière.—The first volume has just appeared and it announces that the second will treat particularly of the *applications of electricity*. This work is beyond doubt the most complete that we have on the subject, and the name of the author, de la Rive, is sufficient to assure us that the facts have been experimented upon by himself, and that nothing is given without good authority.

*Mécanique Analytique par* LAGRANGE, 3d edit., revue et annotée par BERTRAND. 2 vols. Paris, chez Mallet-Bachelier.—Lagrange published the first edition of his work in 1788. In 1811, the principles and general applications, contained in the first edition, were extended and completed. Some obscure points remained, which one of our youngest geometricians, Joseph Bertrand, Examiner at the Polytechnic School, has elucidated with skill, profiting by the progress which the science had made since that period. The edition of 1811 having been exhausted, the new edition has been received with much favor by geometricians and mechanicians.

*Leçons sur la Theorie mathematique de l'élasticité des corps solides*, par LAMÉ; 1 vol. 8vo, avec planches. Paris, chez Mallet-Bachelier.—M. Lamé, one of the most learned geometricians of the Academy of Sciences, has succeeded in rendering the mathematical theory of elasticity as exact and as rigorous as rational mechanics. This work is addressed to engineers, physicists, geologists, and especially to practical men in these arts, who will find in it, rigorous laws on the action of elastic forces, and on the best plans for constructions.

*La règle à calcul expliquée, ou guide du calculateur à l'aide de la règle logarithmique à tiroir*, par BENAIT. 1 vol. in 12mo, avec planches. Paris. Ches Mallet-Bachelier.—This work treats of the methods of constructing the sliding computing scale, and of its use in calculations.

## SCIENTIFIC INTELLIGENCE.

### I. Chemistry and Physics.

1. *Artificial production of polychroism in crystallized substances.*—Senarmont has communicated to the Academy of Sciences the results of experiments upon this subject which are very unexpected and important. The capital fact which the author has discovered is, in his own words, expressed as follows: "A coloring matter disseminated continuously in the interior of a crystal, between its layers of increment but absolutely foreign to its substance, chemically inert and capable of being spontaneously eliminated by successive solutions and crystallizations in pure water, may nevertheless communicate to it in the highest degree the properties of polychroism, and an energy of absorbing action comparable if not superior to that of substances naturally colored in which it shows itself in the most decided manner." As a proof of the correctness of this assertion, the author exhibited large crystals of nitrate of strontia formed in a concentrated tincture of campeachy wood rendered purple by a few drops of ammonia. In these crystals white light developed by transmission under certain incidences, a red color, and under others a blue or violet. Observed with a doubly refracting prism the crystals gave two images, the one red and the other dark violet, according to the thickness, and these images exchanged their colors, passing through identity, as the crystallized plate was made to turn in its own plane. Two similar and perfectly transparent laminæ superposed with a parallel orientation allowed a portion of the incident white light to pass with a purple color; superposed with a right angled orientation, they arrest the light like tourmalines, or at least reduce it to a violet shade so obscure that we may consider the light as extinct. Finally we may detach from these crystals perfectly pure and homogeneous plates slightly inclined to the optic axes. By placing such a plate very near the eye and using white natural light, we see alternately in the direction of each of the axes, a brilliant orange spot traversed by a hyperbolic branch. These open to the right and left of the principal section under the form of curved brushes composed of two equal parts of violet and sombre blue and dividing the field of the plate into two regions in which the purple tints regularly degenerate on both sides of their common limit. The dark tufts interrupted by the luminous spot are moreover fringed towards the point with a little yellow and blue, colors which are altogether local, and which arise manifestly from the dispersion of the optic axes corresponding to the different colors. These phenomena are characteristic of polychroism in crystals with two optic axes, and perfectly similar to those which Brewster observed in Cordierite, and Haidinger in Brazilian Andalusite. The author obtained similar results with other coloring matters and other crystals, and promises more ample details hereafter.—*Comptes Rendus*, xxxviii, 101, *Janvier*, 1854.

[*Note.*—The results obtained by Senarmont clearly demonstrate that the existence of polychroism in crystals by no means necessarily implies their *chemical* homogeneity, since this polychroism may in any

case be produced by the mechanical admixture of foreign coloring matter. Is it not possible that the difficulty of expressing the composition of certain minerals by chemical formulas may arise from such an admixture of coloring matter, the optical characters of these minerals having hitherto entirely misled us?—W. G.]

2. *Rate of transmission of impressions made upon the nerves.*—HELMHOLTZ has communicated to the Physico-Agricultural Society of Königsberg a paper on the methods of measuring very small portions of time, and on their application to physiological purposes. The author alludes in the first place to the remarkable difference observed by astronomers between the observations of different individuals and termed by them the personal equation. The measurements of each observer agree well among themselves, but differ more or less by a constant quantity from those obtained by other persons. The apparatus of Siemens for the measure of the velocity of a musket or cannon ball is next described in its general features. This apparatus only differs from the common revolving cylinder and point now so well known in this country, by the arrangement of this point, which is here fixed, and presses against the surface of the cylinder: when an electric spark passes from the point to the cylinder it leaves a fine dark spot. The passage of the ball establishes metallic connection so that the discharge takes place at the instant of this passage. In this manner the time during which the ball traverses a space of half a line may be measured. The author next alludes to the principle of the revolving mirror due to Wheatstone, and used with so much success by Fizeau and Foucault. Finally he gives an account of the method of Pouillet as modified and used by himself. This consists in causing the galvanic current to act upon an oscillating magnet, observing this magnet by Gauss and Weber's method, and determining the constant factor necessary to convert differences of oscillation into differences of time. In this manner accurate determinations could be made up to the $\frac{1}{10000}$th of a second of time. The physiological questions which the author sought to solve were these. In the transmission of intelligence, is a measurable time necessary for the ends of the nerves to communicate to the brain the impression made upon them; and on the other hand, is time required for the conveyance of the commands of the will from the brain to a distant muscle? By operating with the muscle of a frog severed from the body of the animal but connected with the nerves proceeding from it, the author found that the activity of the muscle is by no means instantaneous, but appears sometime after the excitation of the muscle, increases gradually to a maximum, and then sinks, first quickly, afterwards slowly, so that while the greater part disappears in about one-third of a second, the remaining portion requires several seconds afterwards. The object was to show that the different stages of activity of the muscle take place later when the excitation has to pass through a greater length of nerve, and this is actually the case. The most probable value of the velocity of propagation in the motor nerves of the frog was found to be 26·4 metres or about 80 feet per second. The results of the author's experiments upon the human subject were as follows: The intelligence of an impression made upon the ends of the nerves in communication with the skin is transmitted to the brain with

a velocity which does not vary in different individuals, nor at different times, of about 60 metres, or 195 feet per second. Arrived at the brain an interval of about $\frac{1}{10}$th of a second passes before the will, even when the attention is strung to the uttermost, is able to give the command to the nerves that certain muscles shall execute a certain motion. This interval varies in different persons, and depends chiefly upon the degree of attention. It varies also at different times in the case of the same person. When the attention is lax, it is very irregular, but when fixed very regular. The command travels probably with the above velocity toward the muscle. Finally about the $\frac{1}{100}$th of a second passes after the receipt of the command before the muscle is in full activity. In all therefore from the excitation of the sensitive nerves till the moving of the muscle $1\frac{1}{4}$ to two-tenths of a second are consumed. [For more ample details and for much matter of a highly suggestive character, we must refer to the original lecture.]—*L. & E. Phil. Mag., Nov.*, 1853.

3. *Preparation of large crystals of sulphate of iodo-quinine for optical purposes.*—HERAPATH has succeeded in obtaining crystals of this very interesting and valuable substance of sufficient size to be substituted for tourmaline in polarizing light. For the details of the process however, we must refer to the original paper.—*L. & E. Phil. Mag., Nov.*, 1853.

4. *On the law of induction in magnetic and paramagnetic substances.*—PLUCKER has communicated an elaborate memoir on this subject from which we shall content ourselves with abstracting the summary of results. These are as follows:

(1.) In all magnetic and diamagnetic substances the same general law gives the intensity of the induced magnetism as a function of the exciting force. For each substance this law is particularized by the values of two constants. Of these two constants the one gives, as the inducing force vanishes, the ratio of this force to the induced magnetism (constant of induction), and the second determines the resistance which prevents the induced magnetism from increasing proportional to the inductive force (constant of resistance).

(2.) For every substance there is a point of saturation to which it constantly approximates as the inducing force increases.

(3.) Diamagnetic substances (Bismuth, Phosphorus,) so far as the law of the intensity of their excitation is concerned, behave precisely like magnetic substances, though they exert a directly opposite action upon the inducing magnetic pole. This similar behavior compels us, in my view, to assume that the condition of a diamagnetically excited body is in itself in no wise different from the condition of a magnetically excited body, that furthermore polarity is also present in the excitation of diamagnetic substances, but that this is called forth by an induction which is the opposite to that which occurs in magnetic bodies.

(4.) The curves which represent the law of induction for diamagnetic substances are surrounded on both sides by the curves for magnetic substances. They show that the resistance which is opposed to the excitation of diamagnetic bodies is less than in most magnetic substances, but by no means vanishes; on the contrary it is greater than in oxygen and hydrate of oxyd of cobalt.

(5.) There can be no assumption of a specific magnetism in a substance in a general signification as we speak of specific gravity. Cobalt is precisely as magnetic as iron, when we use a definite magnetic force, which in intensity lies between that produced by one and that produced by two Grove's cups. Iron is more strongly magnetic with a greater, cobalt more strongly magnetic with a less magnetic force. If we compare the specific magnetism of hydrate of oxyd of cobalt with that of hydrate of oxyd of nickel, we obtain numbers which differ from each other by the multiple $2\frac{4}{5}$, according as we use a single element or a battery of 16 elements. The specific magnetism (iron as the point of comparison) of the hydrate is with the last exciting force twice as great as with the first. For oxygen and bismuth this ratio descends to about 1·9 and 1·8. This partly explains the fact that while Faraday's estimate of the magnetism of oxygen, with a nearly equal force and similar mode of observation, corresponds well with my own, other physicists find much smaller numbers for oxygen and bismuth.

(6.) The alternation between magnetic attraction and diamagnetic repulsion in mixtures of magnetic and diamagnetic substances, is completely explained by the change of the specific magnetism with the magnitude of the inducing force. When we mix 10 million parts of bismuth with 310 parts of iron, and fill a small flask with the mixture, we observe neither attraction nor repulsion if we apply a current which is twice as powerful as that which corrresponds to a single Grove's element; with a less powerful current the mixture is attracted, with a more powerful one it is repelled. We can calculate the repulsion like the attraction for a given force. When we use a single Grove's cell, the attraction is nearly one-third as great as the repulsion which, with an equal force, would occur if the whole vessel were filled with bismuth. By employing a battery of 6 elements, we obtain in place of the attraction just determined a nearly equal repulsion. With a battery of 16 elements we obtain finally a repulsion about $\frac{2}{3}$ths of that which pure bismuth experiences with a single element. These results would appear yet more striking if bismuth were mixed with nickel instead of iron. They would make their appearance in the opposite order if the metal were mixed with hydrate oxyd of cobalt.

(7.) Those substances which oppose a lesser resistance to magnetization appear also more easily to retain the magnetism once received. At least we find it asserted of pure nickel that it retains no magnetism while one hydrate of oxyd of cobalt does this; the same is the case with oxygen, which as the author first remarked is attracted or repelled at pleasure by changing the poles of the electro-magnet when contained in an indifferent glass sphere.

In conclusion the author promises us a memoir on the nature of the coercive force.—*Pogg. Ann.*, xci, 1, *Jan.*, 1854.

5. *On the laws of the attraction of electro-magnets.*—Dub has published a continuation of his interesting and valuable researches on magnetic forces. Before stating however the results of this investigation, we must premise that the author distinguishes between magnetism, attraction, and sustaining power in electro-magnets. By magnetism he understands the magnetic excitation of a piece of soft iron by the galvanic current; Lenz and Jacobi measured this by means of the induced

current excited by the vanishing of the magnetism, to which it is proportional. When a second bar of soft iron is caused to approach the first, this also becomes magnetic and by $n$-fold *magnetism*, $n^2$ times the *attraction* is produced. As however the author's experiments have shown that in immediate contact the attraction is not proportional to the squares of the currents it is clearly necessary to distinguish between these two cases of attraction in contact and attraction at a distance. The author gives the following summary of his results:

(1.) The attraction of U-shaped electro-magnets with an equal number of windings of the electro-magnetic spirals is proportional to the squares of the magnetizing current force.

(2.) The attraction of U magnets is, with equal currents, proportional to the square of the number of windings of the magnetizing spirals.

(3*a*.) The attraction of U magnets is proportional to the square of the current-force multiplied by the square of the number of windings.

(3*b*) The *attraction* and the sustaining force both of straight magnets and U magnets is proportional to the square of the current-force multiplied by the square of the number of windings.

(4) The magnetism of massive cylinders of iron of equal length, which are magnetized by galvanic currents of equal force, and by spirals of an equal number of windings closely surrounding the core, is accurately proportional to the square roots of the diameters of these cylinders.

(5.) For the particular case in which the surface of contact does not disturb the result, the attraction and the sustaining force are, with equal magnetizing forces, proportional to the diameters of the bar or U magnets.

(6.) The attraction of bar and U electro-magnets, with equal magnetizing forces, increases the nearer the whole of the windings are to the poles.

(7.) The attraction, like the sustaining force of U electro-magnets, *ceteris paribus*, remains the same whatever be the distance of the branches of the magnet.

(8.) The length of the branches of a U electro-magnet has no influence on its attractive or sustaining force if the windings of the spiral surround its whole length.

In addition to these laws the author has found that the attraction which a helix or spiral exerts upon a soft iron bar placed in its axis follows the same law as an electro-magnet, so that we have

(9.) The attraction of a spiral is proportional to the square of the magnetizing current multiplied by the square of the number of windings.—*Pogg. Ann.*, xc, 248, 433, *Oct. and Nov.*, 1853.

6. *Identity of Niobium and Pelopium.*—H. Rosé has communicated to the Royal Academy of Sciences at Berlin a memoir in which he admits the identity of these metals. The author alludes in the first place to the extraordinary difficulties which have accompanied his investigations from their outset several years since. In earlier memoirs the analogy between pelopic and tantalic acids had been pointed out, as well as the marked difference between pelopic and niobic acids. Continued researches have, however, completely established the fact that pelopic and tantalic acids are essentially different, while between

niobic and pelopic acids there exists a remarkable and unexpected connection. The remarkable peculiarities of the chlorids of niobium and pelopium long since attracted the author's attention. It will be remembered that the only method yet given for the separation of these metals consisted in taking advantage of the difference in the volatility of their chlorids. Chlorid of pelopium being orange red and more volatile than chlorid of niobium. Every time, however, that the white chlorid of niobium was converted into niobic acid, mixed with carbon and heated in a current of chlorine, a quantity of the orange red chlorid of pelopium was formed, and it finally appeared after innumerable most careful and laborious experiments, that the one chlorid became converted into the other by absorbing or losing chlorine; chlorid of pelopium always yielding chlorid of niobium in greater or less quantity. A small quantity of niobic acid considered perfectly pure was mixed with a very large quantity of sugar and gradually carbonized. The carbonaceous mass was again mixed with sugar and charred, and this operation was repeated three or four times to obtain the most intimate mixture of the acid with the carbon. The mixture was then introduced into a glass tube, a current of dry carbonic acid gas passed over it to expel all the air and then heated in a rapid current of chlorine gas. Under these circumstances the niobic acid yielded no chlorid of niobium but only the purest yellow chlorid of pelopium. After several experiments the author found that in order to obtain pure chlorid of pelopium from niobic acid the following precautions were necessary. 1. A large proportion of carbon in comparison with the acid applied. 2. A very careful expulsion of all moisture by strongly igniting the mixture in dry carbonic acid. 3. A complete expulsion of the carbonic acid after the mixture had been cooled in this gas by a very rapid current of chlorine which was only introduced after all the atmospheric air had been driven out of the chlorine apparatus. 4. Finally a very gentle heating after all the parts of the apparatus were so filled with chlorine that they appeared intensely green. The decomposition of the chlorid of pelopium by water yields pure pelopic acid. When any one of the above mentioned precautions is omitted, we obtain at once the white and the orange chlorids. From these experiments it is clear that niobic and pelopic acids are derived from one and the same metal. It is however very remarkable and wholly without analogies among other oxyds that the two acids cannot be transformed into each other by oxydation or reduction. As the quantity of chlorine in the so-called chlorid of pelopium is greater than in the chlorid of niobium, it follows that the two oxyds resulting from these chlorids are different stages of oxydation, and not merely isomeric or allotropic. The author does not give the proportion of the oxygen in the two acids, but simply states that it is very anomalous and only to be compared with the oxygen ratio in two of the acids of sulphur. As the two acids have the same metallic radical it becomes necessary to select a single name, and the author chooses niobium. The former pelopic acid now becomes niobic acid as the highest state of oxydation, but the author does not propose any name at present for the former niobic acid.—*Pogg. Ann.* xc, 456, *Nov.*, 1853.

7. *Preparation and Properties of metallic Aluminum.*—St. Clair Deville has communicated a memoir on aluminum which contains some new facts but which does not add enough to our positive knowledge to justify the extraordinary flourish of trumpets with which the communication was made and received. The metal was prepared by Wöhler's method, namely, by heating the chlorid with sodium, and afterwards fusing the globules into one mass under the mixture of common salt and chlorid of aluminum. As thus prepared it was silver-white, malleable and ductile, and had the fusing point of silver. Its hardness was increased by hammering but it again became soft on heating. Its density was 2·56; it was a good conductor of heat and could be fused and poured out in the air without becoming sensibly oxydized. Aluminum is completely malleable in air dry or moist. Sulphuretted hydrogen, hot and cold water, nitric acid weak or concentrated, and dilute sulphuric acid have no action upon it. Its true solvent is muriatic acid with which it evolves hydrogen, sesquichlorid of aluminum being formed. Heated to redness in muriatic acid gas it yields dry and volatile sesquichlorid. The author stated that the chlorid of aluminum was acted upon by common metals at high temperatures and hoped that further experiments would point out a simple and cheap method of procuring in large quantities and at a low rate a metal so likely to be useful in the arts. The Academy unanimously voted that a sum of money should be placed at the disposal of M. Deville to aid him in the prosecution of his experiments.—*Comptes Rendus, Feb. 6th,* 1854.

[*Note.*—The properties of aluminum as described by Deville, differ in some particulars from those of the metal obtained by Wöhler. Thus Wöhler's metal slowly evolved hydrogen when placed in boiling water and was very readily dissolved by a dilute solution of caustic potash. Further investigations will doubtless explain this difference. The preparation of metallic aluminum in quantity is particularly interesting in a purely chemical point of view, since it will enable us to determine by direct oxydation the equivalent of aluminum which is by no means to be regarded as satisfactorily determined at present.—W. G.]

8. *Preparation of Aluminum by electric deposition.*—Gore has addressed to the editors of the L. and E. Phil. Mag., a letter in which he asserts that aluminum and even silicon may be thrown down from their solutions in a coherent state by feeble electric currents. Hydrate of alumina was dissolved in muriatic acid and a porous cup containing dilute sulphuric acid placed in the mixture. A plate of zinc was placed in the acid and a plate of copper in the solution of alumina, the two being connected by a wire. After a few hours a lead colored deposit of aluminum (?) was obtained, which when burnished resembled platinum. The result was hastened by warming the solutions, and the experiment succeeded with other solutions of alumina. With a small Smee's battery the effect was produced much more rapidly.

When a solution of soluble glass was employed, a single Smee's battery threw down a nearly silver-white metallic film which the author considered to be silicon, but which certainly possesses no analogy with the silicon obtained by purely chemical processes.—*L. and E. Phil. Mag., March,* 1854.

9. *On the arsen-ethyls.*—LANDOLT has studied the combinations of ethyl with arsenic, which are obtained by the action of iodid of ethyl upon an alloy of arsenic and sodium. Of these compounds two, namely, arsenbiethyl and arsentriethyl, are to be regared as analogues of kakodyl, while the third, arsenethylium, is an ammonium in which nitrogen is replaced by arsenic and hydrogen by ethyl. We omit the details of the mode of preparing and purifying these interesting compounds, and pass to the description of their constitution and properties. Arsenbiethyl is a faintly yellow colored liquid, which strongly refracts light, and which possesses an extremely disagreeable penetrating odor of garlic. It boils between 185° and 190° C. Exposed to air it bursts into pale flame, giving out vapors of arsenous acid. When the combustion is imperfect, a red substance corresponding to Bunsen's Erytharsin is always produced. The solution of arsenbiethyl reduces the solutions of the noble metals immediately. The radical unites quietly with the haloids as well as with oxygen and sulphur; its constitution is represented by the formula $As(C_4H_5)_2$, and it consequently corresponds to kakodyl, ethyl here replacing methyl. All the compounds of this radical are fluid, and all possess the same offensive and persistent odor. Arsentriethyl is also a colorless liquid, soluble in ether and alcohol, but insoluble in water. It fumes in the air, but does not generally inflame unless gently heated. The solution does not reduce the oxyds of the noble metals. The formula of this radical is $As(C_4H_5)_3$; it unites with 2 equivalents of oxygen, sulphur, chlorine, &c., and its oxyd saturates two equivalents of acid. The oxyd is a colorless oily liquid. Arsenethylium is formed when iodid of ethyl is brought into contact with arsentriethyl. The radical unites with 1 equivalent of chlorine, iodine, bromine, &c., and gives with 1 equivalent of oxygen a powerful base which forms with acids both acid and neutral salts. These compounds are remarkably susceptible of crystallization, and remain unchanged in the air. They are all without smell, have a bitter taste, and do not appear to possess poisonous properties. The oxyd of arsenethylium is a white mass which has a strong alkaline reaction, separates ammonia from its combinations in the cold, and precipitates the earths and heavy oxyds from their salts. The formula of the crystallized chlorid is $As\,Ae_4Cl+8aq$. The bisulphate has the formula $As\,Ae_4O,SO_3+HO,SO_3$.—*Journal für praktische Chemie*, lx, 385.

10. *On the alcohol of benzoic acid.*—CANNIZARO has found that the oil which results from the action of an alcoholic solution of caustic potash has the constitution $C_{14}H_8O_2$. It is colorless, heavier than water, refracts light strongly, and boils at 204° C. In its relations to reagents it behaves like an alcohol the aldehyd of which is represented by oil of bitter almonds. By the action of nitric acid at a gentle heat the new alcohol is converted into oil of bitter almonds; the action of chromic acid converts it into benzoic acid. The vapor of the alcohol passed over red hot platinum sponge yields an oil which is specifically lighter than water and is probably $C_{14}H_6$. By passing muriatic acid gas into the alcohol the liquid separates into two layers, of which the upper is the chlorid $C_{14}H_7Cl$. This is a highly refracting, strong-smelling liquid, heavier than water, and boiling between 180° and 185°. With caustic potash it gives chlorid of potassium and the alcohol is regene-

rated. Warmed with an alcoholic solution of ammonia, the chlorid gives sal-ammoniac and a crystallizable base which fuses at a higher temperature than Toluidin. By mixing a solution of the alcohol in acetic acid with a mixture of sulphuric and acetic acids, an oil is obtained which is the acetic ether of the new radical, $C_{14}H_7O+C_4H_3O_3$. This is a colorless liquid, having an aromatic odor, and boiling at 210°. With caustic potash it yields acetic acid, and the alcohol.—*Ann. der Chemie und Pharmacie*, lxxxviii, 129, *October*, 1853.

11. *Formation of nitruret of benzoyl from hippuric acid.*—Limpricht and von Uslar have found that when hippuric acid is heated in a tubulated retort, the acid fuses at 130°, gives off a little benzoic acid at 210°, and boils at 240°. The only volatile products of this distillation are benzoic acid, colored faintly red, traces of prussic acid, and a liquid which proves to be nitruret of benzoyl; $C_{14}H_5N$. The constitution and properties of this body correspond perfectly with those of the nitruret obtained by Fehling by the distillation of benzoate of ammonia.—*Ann. der Chemie und Pharmacie*, lxxxviii, 133.

W. G.

12. *On the exhibition of the fixed lines of the solar spectrum with ordinary flint glass prisms;* by Ogden N. Rood.—Herschel in his treatise on light, when speaking of the method of displaying the fixed lines in the solar spectrum remarks: "The slightest defect of homogeneity in the prism, however, as may readily be imagined, is fatal. With glass prisms of our manufacture it would be quite useless to attempt the experiment." This would appear to account for the generally diffused opinion, that the fixed lines in the solar spectrum cannot be seen except by means of "a prism of incomparable flint glass." Not being possessed of a prism of even *comparable* flint glass, I was led to try what could be done with articles of poorer quality. Two flint glass prisms having angles of 60°—such as are ordinarily sold by opticians, and filled with striæ from one end to the other as such prisms almost invariably are, with the extra addition of numerous scratches on the surface and a polish somewhat dimmed—were employed. The prism was placed twelve or fifteen feet distant from a slit $\frac{1}{50}$th of an inch in diameter, through which sunlight was admitted; the room not being darkened. In this manner about twelve or fifteen lines could plainly be seen by the naked eye: the principal of these were D in the orange, F and another line in the green, the strong line in the blue, and two very strong lines in the violet: by more careful manipulation with the prism about thirty lines could be seen and counted. When this spectrum was magnified by a small telescope placed behind the prism, with the exception of the red space the spectrum was filled with lines from one end to the other. It must be remarked however, that very much more careful manipulation is required when employing the telescope: it sometimes being necessary to work for thirty minutes before defining the lines, whereas with the naked eye half a minute will suffice. The angular inclination that succeeds best with the naked eye will not answer for the telescope: that is, for the naked eye the prism should not be placed at its angle of minimum deviation, but the spectrum should rather be expanded.

Light from a white cloud was next employed with the same prism, the slit having a diameter of $\frac{1}{20}$th of an inch, and being distant from the prism about 12 feet. D, F, with three or four of the lines to the left of F, and the strong line in the blue, could thus easily be made out with the naked eye: when magnified by the telescope about forty lines could be counted. In each case, the lines were shown to a number of persons, and in all the cases enumerated except one, colored drawings were made on the spot. With these two prisms fixed lines could be seen in the spectrum from a strip of white paper $\frac{1}{20}$th of an inch in diameter, placed on a black ground and illuminated by sunlight; or better in the spectrum from a white cord $\frac{1}{20}$th of an inch in diameter stretched across a black ground and illuminated by sunlight. In this manner, D, F, with several other lines in the green, and the strong line in the blue, can easily be made out, the prism being held in the hand before the eye, and parallel to the cord.

In order to ascertain the degree of imperfection necessary to prevent prisms showing the fixed lines, thirteen prisms taken indiscriminately from candelabra, and for optical purposes execrable, as all such ornamental prisms proverbially are, were examined. These prisms were ground at angles of 45° and 90°, but were so filled with striæ that *none* of them would give a defined image by total reflexion, and some would scarcely give an image that could be recognized. Sunlight was employed, the slit, &c. being as above: to the naked eye one of the prisms exhibited five fixed lines, two of the prisms showed four lines, four of the prisms showed three lines, four showed two lines, and the remaining two showed one line.

When the light from a white cloud was used, eleven of these prisms showed two lines, viz. F and the strong line in the blue; two showed three lines, viz. D, F, and the line in the blue. In examining the light from the clouds these prisms were not placed at more than four feet from the slit.

It would seem from these experiments then, that the real difficulty is to find a flint glass prism which will *not* exhibit some of the fixed lines, provided its dispersive power is sufficiently high.

New Haven, Feb. 28, 1854.

## II. Mineralogy and Geology.

1. *Appendix to Observations on the Homœomorphism of some Mineral Species;** by James D. Dana.—In order to exhibit the true crystallographic relations of hexagonal and dimetric mineral species, they are presented together in the following tables. The hexagonal species are in five groups, and the relations of these groups are shown by the symbols of planes at the head of the column.

As dimetric and hexagonal forms are alike in having the lateral axes equal, the relation between the crystals of these two systems are best shown by the inclination of the base of the prism on a pyramidal plane. The calculated axes deduced from these angles may diverge, when these angles are the same, since in one system the lateral axes are regarded as crossing at angles of 60° and in the other at angles of 90°.

* Page 210 of this volume.

If we conceive of an ellipsoid of revolution tangent at its sides to the several faces of the prism, as the molecule of each form, it is plain that the angles alluded to afford a correct exhibition of the relations of such molecules as respects their dimensions, and therefore a true representation of the relations of the forms.

There are blanks in the Tables, which are easily filled by calculations according to the usual methods in crystallography.

Very many of the cases of homœomorphism here shown, both in the dimetric system and the hexagonal, and also in the groups on pages 211 to 213, have been brought out from time to time by different authors. The writer has simply endeavored to extend somewhat the groups, and exhibit their relations. The Corundum group was first brought to its present extent by the investigations of G. Rose, who added to it the rhombohedral metals, with tetradymite and red zinc ore. The earliest recognition of the relations of corundum, specular iron and ilmenite, was made by the distinguished chemist and mineralogist of Munich, Prof. von Kobell, in an important paper in the Jahrbuch der Chemie und Physik of Dr. F. W. Schweigger-Seidel for 1832,* in which he was also the first to point out the homœomorphism of apophyllite and anatase; of copper pyrites, braunite, scheelite, scheeletine, wulfenite, cerasine, idocrase, uranite and mellite, besides other facts of interest.

The following important conclusions flow from the Tables.

A. Hexagonal System.—(1.) The vertical axis in Section II is to that of Section I as $1\frac{1}{2} : 1$. The species of the two Sections may be regarded as closely related in form if not homœomorphous.

(2.) The vertical axis of Section IV is to that of Section I nearly as 3 : 1, showing again a simple mathematical relation, if not pointing to actual equality.

(3.) The fundamental pyramid of Section V, is nearly identical in angle with the intermediate pyramid 2-2 of Section I, $\frac{4}{3}$-2 of Section II, in $\frac{2}{3}$-2 of Section IV.

(4.) The Rhombohedral angle of Section IV, is near 90°—or mostly between 84° and 88°; and this approximation to the angle of a cube is its *characteristic*. The pyramid 1(1P) should consequently approach 109° 28′, (the angle of a monometric octahedron,) in its basal angle; it actually varies for the species from Corundum to Iridosmine from 113° to 117°.

(5.) In Section V, the basal angle of pyramid 1 is near 90°, and this appears to be the *characteristic* of the species. Hence the Rhombohedral angle of the Calcite series is near 105°. Hence this rhombohedral angle is a common one among rhombohedral species.

(6.) Section III in its rhombohedral angle approaches 90° as nearly as Section IV, but differs in having the angle greater than 90° instead of less. Its vertical axis is most nearly related to that of Section I, it being approximately *twice* as long as in that Section.

* Neues Jahrbuch der Chemie und Physik, of Dr. F. W. Schweigger-Seidel, Band iv, Heft. 7, 1832; paper entitled, *Beitrag zur isometrischer und homöometrischer Krystallreihen.* The writer's attention has been directed to this paper since the results on page 210 of this volume were published; but he has not yet been able to consult it, except at second hand.

HEXAGONAL SYSTEM.

| I. | O : 1 | O : 2 | O : 3 | O : 1-2 | O : 2-2 | O : 4-2 |
|---|---|---|---|---|---|---|
| Coquimbite, | 151° 00′ | | | | | |
| Beryl, | 150° 3′ | 130° 57′ | 120° 3′ | 153° 29′ | 135° 4′ | 116° 37′ |
| Pyrosmalite, | 148° 30′ | 129° 13′ | 118° 33′ | 152° 3′ | 133° 18′ | 115° 13′ |
| Eudialyte (R), | 148° 38′ | 129° 21′ | 118° 40′ | 152° 10′ | 133° 27′ | 115° 20′ |
| Dioptase (R), | 148° 38′ | 129° 21′ | 118° 40′ | 152° 10′ | 133° 27′ | 115° 20′ |

| II. | O : $\frac{2}{3}$ | O : $\frac{4}{3}$ | O : 2 | | O : $\frac{4}{3}$-2 | |
|---|---|---|---|---|---|---|
| Apatite, | 150° 35½′ | 131° 35′ | 12 °:6′ | | 135° 41′ | |
| Levyne (R), | 150° 43′ | 131° 43′ | 12 °:4′ | | 135° 50′ | |
| Mimetene, | 150° | 130° 53′ | 120° | | 135° 0′ | |
| Pyromorphite, | 150° 27′ | 131° 26′ | 120° 28′ | | 135° 32′ | |

| III. | O : $\frac{1}{2}$ | O : 1 | O : $\frac{3}{2}$ | O : $\frac{1}{2}$-2 | O : 1-2 | O : 2-2 |
|---|---|---|---|---|---|---|
| Quartz (R), | 147° 35′ | 128° 13′ | 117° 42′ | 151° 12′ | 132° 17′ | 114° 27′ |
| Chabazite (R), | 148° 32′ | 129° 15′ | 118° 25′ | 152° 5′ | 133° 20′ | 115° 15′ |
| Dreelite (R), | 147° | | | | | |
| Susannite (R), | 147° 26′ | 128° 03′ | | 151° 3′ | 132° 7′ | 114° 19′ |
| Cinnabar (R), | 146° 32′ | 127° 06′ | | 150° 13′ | 131° 8′ | 113° 35½′ |
| Zinc, | | 126° 57′ | | | | |

| IV. | O : $\frac{1}{3}$ | O : $\frac{2}{3}$ | O : 1 | O : $\frac{1}{3}$-2 | O : $\frac{2}{3}$-2 | O : $\frac{4}{3}$2 |
|---|---|---|---|---|---|---|
| Corundum (R), | 152° 19′ | 133° 37′ | 122° 26′ | 155° 34′ | 137° 44′ | 118° 49′ |
| Specular Iron (R) | | | 122° 30′ | 155° 37½′ | 137° 49′ | 118° 53′ |
| Ilmenite (R), | | | 122° 30′ | 155° 37½′ | 137° 49′ | 118° 53′ |
| Willemite (R), | | | 122° 18′ | 155° 27′ | 137° 35′ | 118° 41½′ |
| Phenacite (R), | | | 123° 16′ | 156° 15′ | 138° 39′ | 119° 36′ |
| Bismuth (R), | | | 123° 36′ | 156° 31′ | 139° 1′ | 119° 55′ |
| Arsenic (R), | | | 122° 9′ | 155° 20′ | 137° 26′ | 118° 34′ |
| Antimony (R), | | | 123° 32′ | 156° 28′ | 138° 56′ | 119° 51′ |
| Tellurium (R), | | | 122° 24′ | 155° 33′ | 137° 42′ | 118° 48′ |
| Iridosmine (R), | | | 121° 33′ | | | |
| Tetradymite (R), | | | 118° 38′ | | | |
| Red Zinc Ore, | | | 123° 5′ | 156° 5′ | 138° 27′ | 119° 23′ |
| Copper Mica (R), | | | 124° 9′ | | | |
| Brucite (R), | | | 119° | | | |

| V. | O : $\frac{2}{3}$-2 | O : $\frac{4}{3}$-2 | O : 2-2 | O : $\frac{1}{2}$ | O : 1 | O : 2 |
|---|---|---|---|---|---|---|
| Pyrrhotine, | 149° 53′ | | 119° 53′ | 153° 20′ | 134° 52′ | 116° 28′ |
| Greenockite, | | | | 154° 32′ | 136° 23′ | 117° 42′ |
| Breithauptite, | | | | 153° 38′ | 135° 15′ | 116° 46′ |
| Copper Nickel, | | | | 154° 41′ | 136° 35′ | 117° 51′ |
| Iodid of Silver,* | | | | 154° 49′ | 136° 46′ | 118° 0′ |
| Nepheline, | | | | 154° 13½′ | 136° | 117° 22½′ |
| Cancrinite, | | | | 154° 7′ | 135° 52′ | 117° 16′ |
| Parisite, | | | | | (½) 136° 33′ | (½) 117° 50′ |
| Tourmaline, | 149° 10′ | | | 152° 40′ | 134° 3′ | 115° 49′ |
| Calcite (R), | 150° 20′ | | | 153° 45′ | 135° 23′ | 116° 53′ |
| Dolomite (R), | | | | 154° 20′ | 136° 8′ | 117° 28′ |
| Magnesite (R), | | | | 154° 57′ | 136° 56′ | 118° 9′ |
| Spathic Iron (R | | | | 154° 43′ | 136° 37′ | 117° 53′ |
| Smithsonite (R), | | | | 155° 2′ | 137° 3′ | 118° 24′ |
| Dialogite (R), | | | | 154° 37′ | 136° 31½′ | 117° 48′ |
| Pyrargyrite (R), | | | | 155° 32′ | 137° 42′ | 118° 4′ |
| Proustite (R.) | | | | 155° 7′ | 137° 9′ | 118° 19′ |
| Millerite (R), | | | | | 142° 44′ | |

* Relation to Greenockite is shown by Descloizeaux, Ann. d. Ch. et de Phys., [3], xl.

DIMETRIC SYSTEM.

| I. | O : 1 | O : 2 | | | | O : 3 |
|---|---|---|---|---|---|---|
| Tin, | 151° 24′ | 132° 31′ | | | | 121° 26′ |
| **II.** | **O : $\frac{2}{3}$** | | | | **O : 1** | **O : 2** |
| Zircon, | 148° 52′ | | | | 137° 50′ | 118° 54′ |
| Rutile, | | | | | 137° 40′ | 118° 46′ |
| Cassiterite, | | | | | 136° 26′ | 117° 43′ |
| Œrstedite, | | | | | | |
| **III.** | | | | | **O : 2*i*** | **O : 4*i*** |
| Scapolite, | | | | | 138° 38′ | 119° 36′ |
| Sarcolite, | | | | | 138° 25′ | 119° 24′ |
| Meionite, | | | | | 138° 43′ | 119° 40′ |
| Mellilite, | | | | | 137° 43′ | 118° 48′ |
| **IV.** | **O : $\frac{1}{3}$** | | | | **O : $\frac{1}{2}$** | **O : 1** |
| Apophyllite, | 149° 30′ | | | | 138° 32′ | 119° 30′ |
| Nagyagite, | | | | | 137° 30′ | 118° 37′ |
| Uranite, | | | | | 137° 27′ | 118° 35′ |
| Anatase, | | | | | 138° 23′ | 119° 22′ |
| Matlockite, | | | | | 138° 37′ | 119° 34′ |
| Calomel, | | | | | 138° 56′ | 119° 51′ |
| Hausmannite, | | | | | 140° 17′ | 121° 3′ |
| **V.** | | **O : 1*i*** | **O : 1** | | | |
| Romeine, | | 134° 17′ | 124° 35′ | | | |
| Cerasine, | | | 123° 06′ | | | |
| Chiolite, | | | 123° 17′ | | | |
| Braunite, | | | 125° 40′ | | | |
| Idocrase, | | | (2) 123° 27′ | | | |
| Copper Pyrites, | | 135° 25′ | 125° 40′ | | | |
| Tin Pyrites, | | | | | | |
| Mellite, | | (1) 133° 27′ | 123° 16′ | | | |
| **VI.** | | **O : $\frac{1}{2}$** | **O : 1*i*** | | | |
| Scheelite, | | 133° 38′ | 123° 59′ | | | |
| Scheeletine, | | 132° 5′ | 122° 33′ | | | |
| Wulfenite, | | | 122° 26′ | | | |
| Fergusonite, | | | 124° 20′ | | | |

(7.) Sections I, II, IV, may form *one Group;* and Section V a *second* Group. Section III belongs most nearly with the first of these Groups. The species of either Group may not have the vertical axis approximately equal; but when widely unequal, the difference is in some simple ratio. As the fundamental pyramid is so *assumed* often for mathematical convenience, there may be in fact approximate equality in some cases, where there appears now to be a multiple ratio; or there may be a multiple ratio, where there is supposed to be equality.

B. Dimetric System.—(1.) The vertical axis of Section I is to that of Section II nearly as 1 : 1½; of Section II to that of Section IV, as 1 : 2; and as it is somewhat hypothetical whether, in the last two, ½ or 1 be correctly the fundamental plane, the relation may in fact be that of equality. For a like reason, since 2*i* may be correctly plane 1, Sec-

tion III may also fall into the same category; so that Sections I, II, III, IV, may form a single homœomorphous Group.

(2.) Sections V and VI have the same relations to one another as III and IV, and may constitute a Second homœomorphous Group.

(3.) In this Second Group, the basal angle of the fundamental pyramid, 1, is near 109° 28′, and the pyramidal angles also; or in other words, the form approximates to the monometric octahedron, as has been often observed.

(4.) In the First Group, the basal angle of the fundamental pyramid, taking Section II as the type, is near 90°, or varies between 82° and 86°.

(5.) The *first* of the two Groups of Dimetric species, corresponds in molecular dimensions with Section V (or Group II) of the Hexagonal System, and the *second* most nearly with Section IV (or Group I) of the Hexagonal System. It remains to be shown how far this relation is an important one.

2. *On Descloizite, a new Mineral Species;* by M. A. Damour, (from the Ann. de Ch. et de Phys.)—The specimen here described was received at Paris from La Plata among different ores of lead. I received it from Mr. Sæmann, Naturalist at Paris, who has allowed me the pleasure of examining it. The following are its characters:

Crystals 1 to 2 millimeters thick, clustered in a group. Form a right rhombic octahedron, with the basal edges truncated; cleavage none. Mostly enveloped in a reddish clayey material, and implanted on a silicious and ferruginous gangue, and associated with green phosphate of lead, in acicular hexagonal prisms. Lustre quite bright. Color in general deep black; but some of the smallest crystals have an olive tint with chatoyant bronze-like lustre. Translucent on the edges, and the color by transmitted light, brown, inclining to red. On a surface of fracture, the color is zoned with straw yellow, reddish-brown and black; and nearly clear at middle; the brown and black colors are deepest at the extremities. Powder pale brown. Surfaces of the crystal, although shining, are generally striated, rugose, and present numerous little cavities. Scratches calcite, and is scratched by fluor: specific gravity at 15 C., 5·839.

Vertical prism M, 116° 25′; brachydome (top angle) 122° 6′; octahedron, 127° 10′, 88° 18′, 115° 10′, the last angle over M.

Heated in a tube, a little moisture is disengaged at an incipient red heat. B.B. on charcoal it melts, and is partially reduced to a globule of metallic lead enveloped in a black scoria. After cooling on charcoal, the mass is surrounded by a yellow areola. Fused with borax in the reducing flame, it gives a glass of a green color; and on adding a little nitre and heating in the oxydation flame, affords a violet tint, characteristic of oxyd of manganese. With salt of phosphorus affords in the reduction flame a glass of a fine chrome-green color, which becomes orange-yellow in the oxydation flame.

Dissolves in the cold in nitric acid diluted with 6 times its volume of water, and leaves a residue of brown oxyd of manganese, mixed with a variable quantity of siliceous sand from the gangue; the solution is colorless. On adding sulphuric acid to it, affords an immediate precipitate of sulphate of lead.

The following is the mean of two analyses:

| | | Oxygen. | |
|---|---|---|---|
| Vanadic acid, - - - | 22·46 | 5·82 | 3 |
| Oxyd of lead, - - - | 54·70 | 3·92 | 2 |
| Oxyd of zinc, - - - | 2·04 | | |
| Oxyd of copper. - - - | 0·90 | | |
| Peroxyd of iron, - - - | 1·50 | | |
| Protoxyd of manganese, - - | 5·32 | | |
| Water, - - - - | 2·20 | | |
| Chlorine, - - - - | 0·32 | | |
| Sesquioxyd of manganese, insol. in nit. acid, | 6·00 | | |
| Siliceous sand, - - - | 3·44 | | |
| | 98·88 | | |

Excluding the oxyds of manganese, iron, copper and zinc, which are coloring ingredients, the mineral is a vanadate of lead, of the formula $Pb^3\dot{V}$. As this compound is a new one among minerals, the author proposes for it the name of Descloizite, in honor of M. Descloizeaux.

3. *Ancient Lake in the Colorado Desert;* (Commercial Advertiser, California.)—The desert lying on this side of the Colorado river was traversed in the latter part of November, by Lieut. J. G. Parke and party belonging to Lieut. Williamson's Surveying Expedition.

The route pursued was from San Bernardino through the pass of San Gorgonio to the eastern side of the mountains, thence along the northern extension of the Colorado Desert to the emigrant trail leading from the Gila river to San Diego. This is a portion of country never before explored.

Mr. Wm. P. Blake, the geologist of the expedition, made a geological reconnoissance of the country passed over, and has communicated to the *Commercial Advertiser* the following interesting statement of facts which came under his observation.

Our party started from San Bernardino on the 13th of November, and on the 16th encamped at the base of the eastern slope of the mountains, under the peak of Weaver Mt., or San Gorgonio. We had already encountered drift sand in ridges and low hills, and at our camp a high drift partly encircled a rude well dug by Indians in the clay. The well was about twenty-five feet deep, wide at the top, growing small to the bottom, where there was about a barrel full of muddy water; this was reached by means of rude stairs cut in the side of the well. The whole was partly overshadowed by several mezquite trees, growing about the opening. The clay surface, in which this well was dug, was hard and smooth, and the bare and rugged mountain rose abruptly from it, with the line of junction as angular and as sharply defined as it could be with a sheet of placid water.

In the morning we travelled over this floor-like surface, in which our wagon wheels scarcely left an impression, encountering an occasional sand drift which had polished the intervening bare surfaces of clay—when, on turning a sharp projecting ridge of the mountain, our attention was suddenly arrested by a distinct horizontal discoloration of the rocks, extending for a long distance on the flanks of the ridge. On approaching it I found that the difference of color was due to a cal-

careous incrustation over the whole surface of the rocks below the horizontal line. This crust was evidently coralline, and had been deposited under water; in its interstices were small spiral shells, similar to those inhabiting fresh water lakes. On examining the clay, this also was found to contain the shells in great number, and I soon collected six species. The water line at this point was not more than fifteen feet above the general surface. Beyond this point the shells became exceedingly numerous, and portions of the surface were thickly covered with shells like the ordinary "fresh water clam."

We were traveling in a long, gradually expanding valley, extending eastward towards the Colorado, and evidently the northern and western extension of what has been called the "Colorado Desert." The southern side of this valley, along under the bordering range on the south, is thickly inhabited in its western part by Indians calling themselves Cohuillas." We passed large villages of them during the day. They build near springs, and their huts are completely hid in groves of mezquite. The squaws were horridly tattooed as well as painted, and manifested much interest in the advent of our wagons and such a number of "pale faces" to their hitherto almost unvisited retreat. Many of the men had often been into the settlements, and had learned some Spanish and accumulated quite a stock of second-hand clothing. Our coming having been duly heralded by *smokes* and excited express riders from one village to the other, the whole population had collected to gaze upon us in wonder. Old hats and odd buttons seemed to have been in especial demand, and were worn with much dignity on the occasion of our arrival. We camped at their last village and spring for a few hours, and learned the existence of traditions among them concerning an ancient lake. They describe it as a wide sheet of water filling the whole valley; it abounded with geese and ducks; and their fathers lived in the mountains. The waters subsided "*poco poco*," (little by little,) and they moved their villages down to the valley it had left. Suddenly the great waters returned and overwhelmed them, drowning many of their people and driving the rest to the mountains. It is their belief that the waters will again come in.

At this part of the valley its width between the bounding ranges is probably fifteen miles, and it here widens towards the south. Our Indian guide refused to accompany us further, and "*mucho malo!*" was the only information that could be extracted from him regarding the section we were to go over before we could reach the emigrant trail. We kept under the southern mountains as much as possible, and the long horizontal line of an ancient beach was distinctly visible for twenty miles on the opposite side of the valley. On our side it was now high above our heads, following all the irregularities and sinuosities of the rugged granite ridges. Observing a few miles off a place that appeared accessible, I started with an assistant and barometer to ascertain the elevation of the line. Arriving under it, the rocks that had appeared diminutive in the distance were found to be huge blocks of granite, covered with the coralline growth nearly *two feet thick*. Large masses of this had fallen off by its own weight, and rolled to the foot of the cliff. By the barometer, the altitude of this water line is *nearly that of the sea level*. The lowest point of the valley is probably not less than five hundred feet below the water line.

The clay surface that has been mentioned, was deposited by the ancient lake. It is of great thickness, and now forms the substratum of the desert. We found numerous deep ravines extending across our course, deeply cut in the clay by floods from the mountains; they were generally narrow, but had *vertical* banks often twenty to thirty feet high. They could not be seen until we came within a short distance, and this often obliged the train to make long and tedious detours.

The "New River" of which you speak has been well known to the travellers of the desert since its sudden appearance a few years ago. The grass that springs up along its banks has hardly an opportunity to attain its full growth, as it is rapidly eaten off by the large droves of cattle and sheep constantly pouring into California from New Mexico and Sonora.

I have reason to believe that this is not the first instance of the overflow from the Colorado, and that it once flowed in a much larger and stronger stream than now, extending along by the well, known as "*Alamo mucho*," where cottonwoods were growing a few years ago. An Indian now living at Fort Yuma well remembers this stream. At the present time "New River" is not a running stream, except at certain high stages of water in the Colorado river; it then flows backward from the river *towards* the lower parts of the basin, and fills a series of local depressions which retain the water for a long time after the river has subsided. I crossed the dry bed of this stream between the pools known as the great and the little "lagoon," in December last; there was no water in it, and that in the lakes was green and charged with organic matter, but made excellent coffee.

It is probable that a local change of level produced by earthquakes has modified the direction of New River, and the quantity of water admitted to its channel.

Earthquakes are not uncommon in that vicinity. In Nov. 1852, two volcanoes burst out from the surface of the desert, and threw up a large column of steam and showers of mud; the country was well shaken for many miles around, and a portion of Chimney Peak, north of Fort Yuma, thrown down; great fissures in the clay of the old lake were also opened.

I did not find the eastern borders of the ancient lake; it is beyond the Colorado; its extent and boundaries cannot be precisely determined until the maps of the region are completed, but it is probable that its area will not be less than 7,500 square miles.

There were a series of outcrops of marine sedimentary formations rising above the general surface of the lake bed; these were filled with the most extraordinary concretions of all sizes and shapes. The ancient sea-drift abounds with silicified wood and marine fossils, all highly polished by the wearing action of sand. Numerous remarkable examples of its cutting action were seen on solid rocks and the hardest minerals. The whole region is one of peculiar interest, not only to the geologist, but it is highly attractive to the botanist and artist.

The facts that I have here given will be presented more in detail, with others, together with observations upon the agricultural capabilities of the desert, in my official report. I may observe in connection with this subject, that a similiar series of dynamical phenomena are present-

ed on a smaller scale in the valley of the Tulare Lakes, into which the waters of the San Joaquin are said to flow when the river is high.

The Gulf of California has undoubtedly extended far to the North of its present limits. Indeed the great ocean once washed the base of the Sierra Nevada and hid the interior of the country far from the light of day. In the slow, but persistent elevation of the continent since the tertiary era, the waters must have necessarily occupied more and more narrow limits, retiring from the elevated plain to the deep valleys until we find them imprisoned between the mountains of the Peninsula and the shores of Sonora. We cannot say that this elevatory movement has yet ceased; it is more probable that it still continues, although unappreciable during the comparatively momentary existence of man. The lake may have been originally cut off from the upper part of the gulf by submarine elevation; but, as the waters covered the whole surface, the sands could not have been acted on or drifted by the northwest winds to form the supposed barrier, as is suggested by your informant.

4. *Quicksilver Mine of Almaden, California;* by W. P. Blake, (from a letter to J. D. Dana, dated San Francisco, Feb. 14, 1854.)—I visited the Quicksilver mine at New Almaden a few days ago, and took a general view of it, and the establishment for the reduction of the ore. I shall soon visit it again and spend several weeks for the purpose of making a full examination of the geology of the vein.

The "Valley of San José" is properly the southern end of the great valley occupied by the Bay of San Francisco. The ground slopes so gently towards the bay that it is hardly perceptible, and appears like a perfectly level plain; the portion bordering the water is low and marshy, and many thousands of acres are left bare at low tides, the streams wander in crooked channels through the deep clay soil—reminding me of the great salt meadows along the Passaic and Quinnipiac rivers.

At the time of my visit the grass was springing up and the wild oat was covering the smooth round hills with a rich green carpet. The majestic oaks and sycamores that give a park-like character to the landscape were leafless, but the monotony of bare branches was relieved by the thick dark foliage of the evergreen oak—and although the middle of winter, the air was as soft and balmy as in our month of May on the Atlantic coast.

The works (or "Hacienda") where the Cinnabar is reduced are two or three miles from the broad valley, up one of the small side valleys of the mountains. The small village has the name of New Almaden, and it has few inhabitants that are not employed in or about the works or the mine.

The mine is about one mile from the works, and high up on the side of the mountain. The cinnabar is brought down on mules that carry about 200 lbs. each, and make two trips a day.

I expected to find the ore in close relation with the deposit of tertiary age, and among bituminous shales, but in this I was disappointed. All the tertiary beds appear to have been washed away, and hard serpentine and trappean rocks constitute a large part of the ridge in which the ore is found. There are however large outcrops of sedimentary rocks, composed of alternating beds of argillaceous shales and layers

of flint; they are highly-tilted and much flexed. They are unlike any of the tertiary series that I have seen in the state, and I am inclined to refer them to the lower Silurian age.*

The ore appears to be mingled with the strata of this formation in a series of beds and interlaminations.

The mine and works are now under the management and general superintendence of Capt. H. W. Halleck, formerly of the U. S. Engineer Corps. An adit level has been run in for 900 feet, cutting the old workings about 200 feet below the former entrance to the mine; this adit is 10 feet by 10, and is well timbered. A railroad of 4 feet guage has been laid down throughout its length, and all the ore and refuse rock of the mine is brought out in cars that are weighed as they pass the office of the mine, and pass to the dressing floors, or to the attle heap. The irregularities and contorted windings of the excavations of the mine can scarcely be imagined; they extend in all directions. The transit between the upper and lower workings is effected by means of stairs or rude steps cut in the slopes, often replaced by notched poles instead of ladders. The ore has been followed and excavated wherever it could be found, and occasional pillars are left to support the roof.

The prevailing opinion among the miners and others appears to be that there is no regular vein of the ore—that it is a "pocket," a "deposit," &c.

The great width and number of the beds has confounded all ideas of extension or prolongation in any particular direction, and led to the general belief that the bed is "as broad as it is long," and the mode of working conforms to this view. The character of the deposit may not be that of a *true vein*, but that it has a determinate extension and direction I have not the slightest doubt, and I had abundant evidence of the fact. There is no reason why systematic mining should not be practised, both on the score of economy and safety.

The ore is divided into lenticular masses by intercalations of rock of variable thickness, and these are often filled with seams and veins of the sulphuret.

Numerous veins of carbonate of lime traverse the rock in various directions, cutting through the ore and faulting the small veins; it also lines cavities in the masses of cinnabar, and is then finely crystallized, and sometimes contains small quantities of bitumen in cavities, and implanted in globules among the crystals.

Sulphurets of iron and copper and arsenical pyrites are associates of the ore, but occur in very small quantity. Gold is said to have been found frequently in small quantities. I searched for crystals of cinnabar in vain, it is all massive, but presents various shades of color, and fresh fractures possess great brilliancy and beauty.

The mine is so free from water that the vein is scarcely decomposed at any point. If this had been the case, there would probably be some interesting mercurial salts formed along the walls.

The mining is principally performed by Mexicans and by Yaqui Indians. These are from the Gulf of California, and are of the same tribe that are employed in the pearl fishery. They make excellent

* Similar rocks (talcose and argillaceous shales and a Jasper rock) occur north of the harbor of San Francisco; they are more or less metamorphic, and probably *not older* than stated above by Mr. Blake.—J. D. D.

miners, and endure much hard labor. One or two Cornish men had recently commenced work.

The drilling is all done with long heavy drills, and the hammer is not used. I saw one bed of cinnabar about 8 feet thick, into which a powerful Yaqui Indian was drilling a hole; the crimson vermillion mixed with water was flowing from it in a stream and bespattering the surrounding surfaces. It is really delightful to see such quantities of beautiful ore as are displayed in this mine.

I was greatly pleased with the construction of the furnaces and arrangement of the works for reduction. They are an instructive example of the value of science united with practical skill. The superintendent has discarded all the old and complicated methods, and adopted a furnace in which the reduction is rapidly effected without the use of lime, or even crushing the ore. All the vaporized sulphur is converted into sulphurous acid by the admission of atmospheric air, thus separating it from the mercurial vapor, and preventing a recombination in the condensing chambers. Some improvements can yet be made, especially in the apparatus for condensation, as a small quantity of mercury escapes from the chimneys in vapor and deposits the peculiar gray coating on their tops.

I have before me a specimen of cinnabar from a new vein recently discovered in Sonora. It has been found in small quantities only, and is chiefly interesting from the occurrence of metallic mercury in the parts of the vein near the surface. They occupy small cavities in the rock, and are apparently derived from the decomposition of the sulphuret.

5. *Conistonite.*—In a recent letter from R. P. Greg, Jr., (dated Manchester, Eng., March 17,) we have received a drawing of another crystal of Conistonite, which has on the angles, the planes of a brachydome, in addition to those of the figure on p. 133. He gives also the following revised angles, $M : M = 97° 5'$, $M : e = 122° 55'$, $M : c$ (brachydome) $= 117° 30'$, $c : e = 121° 0$, $c : c$ (top) $= 96° 50'$, $c : c$ (base) $= 83° 10'$, $e : e$ (base) $= 93° 30'$, $e : e$ (top) $= 86° 30'$. Faces not very bright, $c$ the brightest. No cleavage observed. When heated it gives off a peculiar pungent argillaceous odor, and something like the smell of burning paper.

### III. Botany and Zoology.

1. *On the Age of the large Tree recently felled in California;* by A. Gray. (Read before the American Academy of Arts and Sciences.) —The age attained by the largest known trees is a matter of considerable interest; but it is seldom that an opportunity occurs of testing it by an actual counting of the annual layers of the trunk. This is said to have been done in the case of the gigantic tree recently felled near the head of the Stanislaus River, on the Sierra Nevada, California, a section of the trunk of which, at 25 feet from the ground, and hollowed out to a shell, is now on exhibition at Philadelphia. The trunk of this tree "was sound from the sapwood to the centre;" and its annual layers are very distinct to the naked eye in pieces of the wood in my possession. The size of this tree is such as to give it a presumptive claim to rank among the oldest of the present inhabitants of the earth; its length being 322 feet; the diameter of the trunk at 5 feet from the ground,

29 feet 2 inches; at 18 feet, 14 feet 6 inches; at 200 feet, 5 feet 5 inches, including the bark. These measurements are copied from Mr. Lobb's account of the tree published in England, except the height (by Mr. Lobb said to be about 300 feet), which I have given on the authority of the proprietor of the section now at Philadelphia. This section was taken at the height of 25 feet from the ground, and, according to the measurement of my friend, Thomas P. James, Esq., of Philadelphia, it is about 12½ feet in diameter, including the bark. Mr. James, at my request, has taken careful measurements of the wood itself, excluding the bark. The three diameters taken by him, respectively measure 9 feet 6 inches, 10 feet 4 inches, and 10 feet 10½ inches; the average diameter of the trunk at the height of 25 feet from the ground, is a little over 10 feet 3 inches. From the statements which have appeared, it would seem as if the layers had actually been counted, and ascertained to be 3000 in number. This surely ought to have been done; but an examination of the statements does not prove that it was. Mr. Lobb's statement, as definite and reliable as any, is, that "the trunk of the tree in question was perfectly solid, from the sapwood to the centre: and *judging* from the number of concentric rings, its age has been *estimated* at 3000 years."

The number of layers, therefore, has only been *estimated;* and we are not in possession of the exact data on which the estimate was founded. The data wanting are the average thickness of the layers towards the centre, giving the rate of the tree's growth as a young and middle-aged tree, when it must undoubtedly, like other trees, have increased more rapidly than in later years.

Dr. Lindley, I find, (in the Gardener's Chronicle), has accredited the estimate which assigns to this tree an age of above 3000 years; stating that "it may very well be true if it does not grow above 2 inches in diameter in 20 years, which I believe to be the fact." That rate would indeed give 3500 layers at the height of 5 feet from the ground, where it is 29 feet 2 inches in diameter. But this measurement appears to include the bark, to allow for which Dr. Lindley would perhaps give up the odd 500 years. There is a further consideration. At 25 feet from the ground the diameter of the wood is less than 10 feet 4 inches. Here the rate of two inches in diameter in 20 years would give the trunk an age of only 1230 years, so that on these data, the tree in its youth would have been 1770 years in adding 20 feet to its stature! It is evident that the trunk is much enlarged at the base, as in many old trees, especially of the same tribe, such as the *Taxodium*, &c.

The section of the trunk at Philadelphia has been hollowed out, by fire and other means, to a shell of 3 or 4½ inches in thickness. Of this I have, through the kindness of the proprietor and of Mr. James, a piece of the wood, including nearly 3 inches of this section. What is now wanted, and what unfortunately I do not possess, is a foot or two of the wood from the central parts of the tree—a desideratum which may doubtless be supplied hereafter. The data at hand, however, will suffice for determining an age which the tree cannot exceed, unless it be supposed to have grown more slowly during the earlier $\frac{9}{10}$ of its existence than during its later years, which is directly contrary to the ascertained fact in respect to trees in general. Now the piece of wood in my hands exhibits an average of 48 layers in an inch. The semi-diameter

of the trunk at the place whence it was taken is about 5 feet 2 inches. If the tree increased in diameter at the same rate throughout, there would have been 2976 annual layers; which, allowing 24 years for the tree to have attained the height of 25 feet, would give it an age of 3000 years from the seed. This corresponds so closely with Dr. Lindley's estimate that we may suppose him to have employed equivalent data in a similar manner. How great a deduction must we make from this estimate, in consideration of the greater thickness of the layers as a younger tree? The only direct data I possess bearing on this point are derived from a piece of a transverse section 3½ inches deep, of a "rail," which the exhibitor says was taken from the trunk at the height of 275 feet from the ground. As its layers, on a breadth of nearly ⅞ths of an inch, show only a slight curvature, it must have come from a part of the trunk still of several feet in diameter. On this section the exterior inch, nearly all alburnum, contains 90 layers; the next 60; the next 45, the remaining half inch 16, making 32 to the inch. That the exterior layers should be thinner at this height than those near the base of the tree, is just what would be expected. If we apply this ratio of decrease of the number of layers to the inch as we proceed inwards, to the section at 25 feet from the ground, we should, at four inches within that part of the circumference which I have examined, have only 17 layers to the inch, which taken as the average thickness, would make the tree only 1034+24=1058 years old. But it is not probable that the thickness of the layers increases so rapidly. The data we possess on other trees goes to show that a tree after it is 400 or 500 years old, increases in diameter at a pretty uniform rate for each 20 additional years, on the whole, although the difference in the thickness of any two or more contiguous layers, or of the same layer in different parts of the circumference, is often very great. Still, when we consider how very much thicker are the annual layers of a vigorous young tree than of an old one, perhaps we should not be warranted in assuming more than the average of 17 layers to the inch for the whole section.

Some useful data may be obtained from a tree more nearly related than any other to those of California, though of a different genus, namely the so-called Cypress of our southern States (*Taxodium distichum*). I possess three sections of different trees of Taxodium, reaching from the centre to the circumference. One of these, on an average radius of 27 inches, exhibits 670 layers; a second, on a radius of 30 inches, has 525; a third, on a radius of 22 inches, has 534 layers. The average is 576 layers to a semi-diameter of 26 inches, or about 22 layers to an inch. Half of this growth (13 inches radius) was attained at the close of the first century; while the exterior layers of the oldest specimen were only the fiftieth or sixtieth of an inch in thickness. We have reason to believe, therefore, that the *Wellingtonia* (as it is called) of California is at least as rapid in its growth as the *Taxodium*. We may safely infer, I think, in the absence of other data, that when the tree in question had attained the size of 26 inches in semi-diameter, it was only 576 years old. If therefore we suppose it to have increased at the intermediate ratio of 35 layers per inch for the next 26 inches, and at the actual rate of the last century (as ascertained by inspection), namely at 48 layers per inch, for the remaining 10 inches, we should assign to it the age of 2066 years as its highest

probable age. I think it more likely to be shown, when the wanting data are supplied, that the tree does not antedate the Christian era. There are said to be 80 or 90 such trees, of from 10 to 20 feet in diameter, growing within the circuit of a mile from the one felled. When the next of these venerable trees are wantonly destroyed, it is to be hoped that its layers will be accurately counted on the whole section, and the thickness of each century's growth carefully measured on the radius.

The tree in question is a near relative of the *Redwood* of California, namely the *Taxodium sempervirens* of Don, of late very properly distinguished as a separate genus under the unmeaning and not euphonious name of *Sequoia*, a tree now grown in England, and sparingly also in our own vicinity, where it is barely hardy. My friend, Dr. Torrey, has for nearly a year possessed specimens of foliage of this tree, which he took to be a new species of *Sequoia*. The fruit and branches of the juniper-like foliage (probably only one form of a dimorphous foliage, which is common in *Cypressineæ*) having been received in England from Mr. Lobb, by Dr. Lindley and Sir. Wm. Hooker, they have recognized in this tree the type of a new genus, distinct from *Sequoia*, to which the former has given the name of *Wellingtonia*. The wood is, I believe, much the same as that of *Sequoia sempervirens*, which tree also attains a gigantic size. The principal characters yet ascertained are that the cones of *Wellingtonia* are oblong and have a thick woody axis. Additional materials are needed to confirm the genus, if such it be.

2. *Synopsis Plantarum Glumacearum, autore* E. G. Steudel, fasc. 1. Gramineæ, p. 1–80. Stuttgart, 1854. Imp. 8vo.—The characters, in this new revision of Grasses, are about as full as those of Kunth's *Enumeratio Plantarum:* the references less full. The large page is compact and in double columns, so that eleven such fasciculi may, as proposed, comprise the known Glumaceous plants. This fasciculus takes in the *Oryzeæ*, the *Phalarideæ* (in which Zea is still included!) and the greater part of the *Paniceæ*,—among them 568 species of *Panicum!* without reaching the end of that genus. A proper study would reduce the number of species fully one-half. A. G.

3. *Lindley's Folia Orchidacea:* part 5, was published in February last. It comprises 8 genera, of which the principal as to number of species are *Brassia*, *Sobralia*, and *Cœlogyne*. A. G.

4. *Epistolæ Caroli a Linné ad Bernardum de Jussieu Inditæ, et mutuæ Bernardi ad Linnæum;* curante Adriano de Jussieu, (Ex Act. Acad. Art. et Scient. Amer. ser. nov. tom. v.) 1854 pp. 51, 4to.—It is somewhat singular that the correspondence of these two founders of modern botany should have remained so long unpublished (excepting translations into English of several of the letters published by Sir J. E. Smith); and it is not a little remarkable that it should at length be given to the world in this country, as a contribution by the lamented editor to the publications of an American scientific society of which he was a distinguished foreign member. The notes furnished by Adrien de Jussieu possess the sad interest of having been the last scientific occupation of the last of the Jussieu's. The publication makes a part of the forthcoming half-volume of the Memoirs of the American Academy of Arts and Sciences. The greater part of the extra copies printed

have been forwarded to the family of the editor for distribution among his scientific friends. A. G.

5. *Localities and Habits of certain species of Insects, &c.;* by J. P. KIRTLAND.—In vol. xiii, page 336 of the Amer. Jour. of Science, were described the Libythea Bachmanii and the Macroglossa balteata, from solitary specimens, captured many years since at Poland, O., little was known respecting them. Additional facts in regard to their localities and habits have been recently obtained.

In June last, while visiting the late Wm. Jenison,* of Dayton, I found several specimens of the first named insect in his collection. He informed me that they visited his gardens every year, in rather limited numbers, but he had never discovered them in their larva state.

Subsequently Dr. Hoy, of Racine, Wis., furnished me with many fine specimens. From him I learn that during a brief period of about two weeks, they resort in great numbers to the flowers of the garden raspberry, (Rubus Idæus), and are seen at no other time. During their sojourning they doubtless deposit their eggs on some species of vegetation, which affords appropriate food for their larva. Whether their resort to the flowers of the raspberry is for that purpose, or to obtain food for themselves, has not been ascertained. It is evident that one brood only can be produced in a summer.

The Macroglossa I found in Dr. Hoy's cabinet, as also in Gov. Farwell's collections at Madison, Wis., and learned that it is occasionally seen in different parts of that state. In its habits it is said to resemble the *Sesia pelasgus.*

From what I have ascertained, I conclude that the Libythea is sometimes found as far east as Boston, Mass., but that both it and the Macroglossa belong more appropriately to sections of the country west of Ohio.

The crustaceous animal, closely allied to the *Palemon vulgaris* of Say, taken by Prof. Baird at Port Sarnia, in Canada, I have recently captured in Rocky River, seven miles west of Cleveland. This animal Prof. Dana has, I believe, decided to be specifically new, and related in genus to Anchistia of the Palæmonidæ.

## IV. ASTRONOMY.

1. *Comet* V, 1853, (Astron. Jour., No. 67.)—This comet, discovered by Mr. VAN ARSDALE, on the 25th of November last, was observed at Göttingen by KLINKERFUES, on December 2. Professor S. ALEXANDER, of Princeton, N. J., has obtained the following elements of its orbit from VAN ARSDALE's observations of Nov. 30, Dec. 27, and Jan. 21.

T. 1854, Jan. 5$^{d}$·77967, M. T. Greenwich.

| | | |
|---|---|---|
| Long. of perihelion, - - | 55° 15′ 7″ | Mn. Eqnx |
| " asc. node, - - - | 227 5 27 | Jan. 1. |
| Inclination, - - - - | 66 11 53 | |
| Log. of perihelion dist. - - | 0·3097879 | |

Motion retrograde.

* Mr. Jenison died in July last. His was a life of vicissitudes. He was at one period a student of Comte Dejean. In the retired situation in which he spent a number of the last years of his life, near Dayton, O., amusing himself with horticulture, and almost every branch of natural science, his amiable disposition, talents and attainments, were neither known nor appreciated. He was an accurate and intelligent entomologist.

2. *Obituary.*—Professor A. C. PETERSEN, (Astron. Jour., No. 68.) —This eminent astronomer has recently closed his earthly labors. For many years he had been connected with the Observatory at Altona. His numerous contributions to the *Astronomische Nachrichten* continued through twenty-five years, attest the extent and value of his labors. On the death of Schumacher, in 1850, Dr. Petersen succeeded him in the direction of the Observatory, and in the editorship of the *Astr. Nach.* "The mantle of the illustrious Schumacher seemed to have fallen upon Petersen. To him, in conjunction with Professor Hansen, was bequeathed the direction of the priceless *Astronomische Nachrichten* to which a large portion of many years of his life had already been devoted. To him was confided also the direction of the Observatory of Altona. And he possessed in a signal degree the noble qualities of heart and soul which have endeared the memory of Schumacher to all who knew him. Warm hearted, unselfish, true, devoted to science, to duty, and to his friends, he has left a void which it will not be easy to fill."

## V. MISCELLANEOUS INTELLIGENCE.

1. *Abstract of Meteorological Observations made at Burlington, Vt., in* 1853; by Z. THOMPSON.—Location, Lat. 44° 29′, Long. 73° 11, one mile east of Lake Champlain and 256 feet above it, or 246 feet above tide.

| 1853. | THERMOMETER. | | | | BAROMETER. | | | |
|---|---|---|---|---|---|---|---|---|
| Months. | Mean. | Highest. | Lowest. | Range. | Mean. | Highest. | Lowest. | Range. |
| | ° | ° | ° | ° | | | | |
| January, . . | 19·63 | 45 | −11 | 56 | 29·76 | 30·45 | 28·54 | 1·91 |
| February, . | 21·47 | 47 | −10 | 57 | 29·68 | 30·13 | 28 86 | 1·27 |
| March, . . | 32·77 | 54 | 1 | 53 | 29·57 | 30·28 | 28·84 | 1·44 |
| April, . . . | 42·58 | 77 | 22 | 55 | 29·63 | 30 00 | 28·70 | 1·30 |
| May, . . . | 55·19 | 87 | 27 | 60 | 29 65 | 30 08 | 29·30 | ·78 |
| June, . . . | 67·27 | 94 | 43 | 51 | 29·72 | 30·02 | 29·40 | ·62 |
| July, . . . | 68·70 | 90 | 50 | 40 | 29·71 | 29·94 | 29·39 | ·55 |
| August, . . | 68·15 | 95 | 44 | 51 | 29·65 | 29·90 | 29·28 | ·62 |
| September, . | 59·48 | 93 | 30 | 63 | 29·70 | 29·97 | 29 10 | ·87 |
| October, . . | 47·02 | 70 | 23 | 47 | 29·67 | 30·14 | 29·09 | 1 05 |
| November, . | 37·27 | 62 | 9 | 53 | 29·93 | 30·50 | 29·09 | 1·41 |
| December, . | 22·94 | 42 | − 4 | 46 | 29·67 | 30 01 | 28·74 | 1·27 |
| Annual mean, | 45 21 | 95 | −11 | 106 | 29·69 | 30·50 | 28·54 | 1·96 |

| 1853. | WINDS. | | | | | | | | WEATHER. | | SNOW. | WATER. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Months. | N. | N.E. | E. | S. E. | S. | S. W. | W. | N. W. | Fair. | Cloudy. | Inches. | Inches. |
| January, . | 12 | 3 | 0 | 1 | 10 | 1 | 1 | 3 | 15 | 16 | 14 | 1·22 |
| February, . | 10 | 1 | 1 | 4 | 9 | 0 | 0 | 3 | 13 | 15 | 29 | 3·94 |
| March, . . | 4 | 1 | 1 | 2 | 8 | 1 | 5 | 9 | 21 | 10 | 14 | 1·70 |
| April, . . | 10 | 2 | 1 | 0 | 11 | 1 | 2 | 3 | 21 | 9 | 14 | 2·25 |
| May, . . | 9 | 1 | 1 | 1 | 15 | 1 | 1 | 2 | 18 | 13 | 0 | 3·95 |
| June, . . | 5 | 0 | 1 | 0 | 16 | 1 | 4 | 3 | 25 | 5 | 0 | 1·74 |
| July, . . | 5 | 1 | 1 | 1 | 13 | 5 | 2 | 3 | 24 | 7 | 0 | 3·12 |
| August, . | 8 | 1 | 0 | 1 | 15 | 3 | 1 | 2 | 26 | 5 | 0 | 3·46 |
| September, | 11 | 1 | 1 | 1 | 10 | 2 | 1 | 3 | 21 | 9 | 0 | 5·67 |
| October, . | 2 | 1 | 1 | 2 | 12 | 1 | 6 | 6 | 20 | 11 | 4 | 3·04 |
| November, | 8 | 1 | 0 | 2 | 11 | 1 | 2 | 5 | 15 | 15 | 7 | 2·17 |
| December, | 10 | 1 | 0 | 2 | 14 | 1 | 1 | 2 | 17 | 14 | 8 | 0·79 |
| | 94 | 14 | 8 | 17 | 144 | 18 | 26 | 44 | 236 | 129 | 90 | 33·05 |

The results given in the above tables are derived from three daily observations, made at sunrise, 1 P. M., and 9 P. M.

The mean temperature of 1853 was one-half of a degree warmer than the average of the preceding 15 years. The warmest day was the 12th of August, the mean heat of which was 83½°; and, on the same day, the thermometer was highest in the shade, standing at 95° at 3 P. M. The coldest day was the 26th of January, which averaged −3°, or 3° below zero; and the greatest cold was −11°, on the morning of January 27th.

The barometer was highest on the 27th of November, when it stood at 30·50 inches, and lowest on the 24th of January, standing at 28·54 inches, showing a range for the year of 1·96 inch.

The range of the thermometer was 8° less, and that of the barometer 0·27 inches greater than in 1853.

The fall of water in rain and snow, was just 1 inch more than the mean annual fall in the preceding fifteen years, (which was 32·05 inches) and 4·23 inches more than in 1852. The fall of water in December, 1853, was less than in any December in the preceding 15 years, and less than half the average for December, that being about 2·5 inches.

The fall of snow was 13 inches less than in 1852, and 19 inches more than in 1851. During the year there were 54 days of tolerable sleighing,—the same amount as in 1852. The broad part of Lake Champlain, opposite Burlington, was closed with ice about the 29th of January, for two days, but opened and did not close again till the 20th of February. From that time it remained closed, for the most part, till the 10th of April; but the ice was, all the while, considered unsafe, and teams ventured to cross only for a short time about the middle of March. Steamboats appeared on the lake the 12th of April. The lake was highest about the 29th of May, and was then 18 inches below the extreme high-water mark of May, 1847. It was lowest on the 8th of September, being then 6 ft. 8 in. below high-water mark, showing a change of level in 1853, amounting to 5 ft. 2 in.

The following indications of the advance of spring were noted:

*March* 22d, Robins and Long Sparrows seen; 25th, Blue Birds, *S. Wilsonii;* 26th, Cherry Birds, *B. Carolinensis.*

*April* 18th, White-bellied Swallow, *H. bicolor;* 24th, Liverwort in blossom, *H. triloba;* 26th, Toads and Frogs heard; 29th, Red Maple, *A. rubrum;* White Elm, *U. Americana;* Barn Swallows, *H. rufa.*

*May* 9th, Tree Toads, *H. versicolor;* 13th, Bobolink, *I. agripennis;* 17th, Plums in blossom; 21st, May Bugs, *M. quercina;* 22d, Pears in blossom; 26th, Crab Apple, Siberian; 27th, Common Apple.

The appearances of the Aurora Borealis at Burlington in 1853, noted in my journal, were as follows:

*January* 4th, A bright glow in the N., but no distinct arch; 6th, ibid; 8th, a distinct arch in N. E. at 7 P. M.

*February* 8th, Glow of light in N. W.; 14th, a well formed arch, 12° high at 9 P. M.

*March* 7th, Faint Aurora at 8 P. M.; 8th, very splendid from 8 to 10 P M.; 10th, ibid, with a well defined flat arch, 15° high at 10 P. M.; 30th, faint.

*April* 1st, Aurora Borealis in N. E., arch 10° high at 9 P. M; 5th, Aurora very splendid, commenced as the clouds dispersed a little before 9 P. M., and increased in brightness till 10. At 9½ it consisted of an exceedingly bright glow in the north, rising nearly to the pole-star. At 10 it rose considerably above the pole, with numerous bright pillars of light lower down; 7th, a very fine Aurora, with the principal arch nearly stationary from 8½ to 10 P. M., and its vertex about 10° high; 10th, faint.

*May* 1st, Faint Aurora low in the N.; 2d, meteor very splendid, at 8 faint, at 8½ a regular arch from E to N. W., 6° above the pole; at 8h 55m a corona formed 8° S. W. of the zenith, with the radiations very distinct and beautiful. The meteor was very changeable, and the streamers at times exhibited a dazzling brightness. 4th, faint Aurora, with no distinct arch; 6th, ibid; 7th, ibid; 31st, ibid.

*June* 2d, Aurora Borealis faint; 8th, ibid; 9th, ibid.

*July* 4th, Faint Aurora; 10th, ibid; 12th, fine Aurora, with streamers flitting rapidly at 9½; 23d, faint Aurora in N. E.

*August* 31st, Slight Auroral light.

*September* 1st, Fine Aurora, commenced at 9 P. M. and continued through the night; 2d, very fine Aurora which continued through the night,—at 9 P. M. there was a flat arch under the pole, 15° high; 3d, another fine Aurora, with a distinct arch under the pole, 25° high at 9 P. M.; 10th, faint Aurora; 24th, ibid.

*October* 31st, Slight Aurora low in N.

*November* 1st, Ibid.

*December* 4th, Beautiful Aurora, bright arch low in the N.; 8th, faint Auroral light in N.

2. *Extract of a letter from Colonel J. C. Fremont, respecting his Explorations for the route of a Railroad to the Pacific.*

[The letter from which extracts are here given was prepared by Col. Fremont for publication shortly before he set out on his journey, but for particular reasons it was not printed at the time. Those reasons no longer exist, and it is now proper that the letter and extracts from the correspondence of Col. F. should be given to the public. The name of the person to whom the principal letter is addressed need not be given.]

"My own journeys through our interior mountains had already in 1847 satisfied me that a direct railroad route ought to be searched for along the parallel of 38° 39′. Information acquired from all sources led me to believe that this range of country was certainly practicable, and that the most important points on either ocean might be connected, by a line which should be direct, and at the same time penetrate the mountains through a region admirably adapted for settlement and cultivation. With this view I had embraced in my original plan of explorations, (and so stated in one of my published journals,) an examination of the central section of the Rocky Mountains; comprehending the Three Parks, with the numerous valleys which enclose the head waters of the South Platte, the Arkansas, and the Del Norte on the one side, and the sources of the East Fork of the Great Colorado on the other.

Since 1847 my attention has been continuously occupied with this line, and with this section of the Rocky Mountains through which I have proposed that the great road should pass, entering the valley of the Colorado through the valley of the San Luis or Upper Del Norte. To this point my own examinations have been extended, with the satisfactory result of finding a way easy and good, through a broad and fertile country, allowing straight roads and choice of lines with continuous and expanded settlements. Such a line seems to comprehend all the advantages which ought to be combined in a trunk road to the Pacific. It is direct, central, and feasible of construction; it commands the most practicable pass, and best known route for a branch road into Oregon, and would be the means of forming a new and great state of this Union—the Switzerland of America.

With these views I attempted, but was prevented from exploring it in 1847. In 1848 I resumed the attempt in an expedition which was defeated in its complete result; but was successful so far as it went, and completely successful so far as it realized my own idea.

About this time finding myself owner of a property which promised to be of extraordinary value, I conceived the idea of wholly devoting it, as far as it would go, to the prosecution of the work; and on my return to Washington in 1850, I communicated it to Mr. Francis P. Blair and other friends, who highly approved of my plan, and through whom, without design on my part, it reached the public press.

In the fulfillment of this plan I visited England with the expectation of engaging foreign capital in the development of the property, so as to make it available to the enterprise. Protracted delays and difficulties growing out of the contest for my property with the government, blunted or paralyzed my efforts, and I had succeeded only to a limited extent, when information of the general movement at home in favor of the railroad reached me. I immediately arranged my affairs for an absence of six or eight months, provided the necessary instruments, and started for Washington. On my arrival I found the [U. States] exploring parties were fully organized, and the Government commands already disposed of. Finding myself prepared with time and instruments, and fairly engaged in the enterprise, which properly formed a continuation of my own plans, I have decided to carry it out by my private means. The property above referred to had diminished in value from the litigations of the United States; much of its absolute and immediately available wealth had been carried away, but it had become better defined, and I had recently received in England an offer of two millions [of dollars?] for half, dependent upon recognition of title. This realization of the property has been again subjected to indefinite delay by the recent appeal of the Government from their own tribunal, but I have decided to invest it, whenever it becomes available, in that part of the road which shall go through the state of California from San Francisco to its western frontier, at whatever point the road from the East should strike it.

Under this deprivation of resources, I can only go on with one branch of my intended enterprise, that of completing my examinations of the country between the Upper Del Norte and the valley of the San Joaquin. Upon this line I propose to make a double examination—

going out before the snows fall, and returning in mid-winter. A winter exploration, in making me acquainted with the depth and prevalence of the snows, and the extent of their impediments to winter traveling, would enable me to judge practically of some points very material to the right decision of the question.

The field of operations reaches over an expanse of mountain wilderness extending the whole length of our domain from north to south. In view of the enduring and unchangeable character of the work, no line can be considered approximately determined until all possible information should have been obtained, so that the best route, under every aspect for the country, may be adopted. To meet the actions of the next Congress, will give ample employment to all the labor that can be brought to it, and my examinations therefore must necessarily contribute to the mass of materials for the solution of the question. Whatever may be the results at which I arrive, they shall be fairly communicated to the public, as an element in aid of their decisions.

Finally, and above every other consideration, I have a natural desire to do something in the finishing up of a great work in which I had been so long engaged. I do it with the object and the hope of adding to the favorable considerations which (I may be permitted to say) have recognized the disposition I had already shown to serve the country. A deference to this favorable opinion, which I should regret by any act to impair, makes the occasion for the present letter. I felt that some explanation was due to the public for taking part as a private individual in this public concern, and was unwilling to leave the motives of the present journey exposed to misconstructions. I judged therefore that a clear statement of them would not be considered improper or uncalled for."

Soon after Col. Fremont started on his journey, illness forced him to return to St. Louis for medical aid. The exposures of the winter of 1848 had fastened upon him a painful affection of the left leg which had become "neuralgic sciatica." He had been free from it for a year or more, but the great change of living seemed to develop it again. After a delay of just a month, he was able to make a second start, leaving Westport the 21st of October. During his illness, his party, which consists of some twenty-two men (half being Delaware Indians,) waited for him about a hundred and fifty miles from the frontier. On rejoining them, Col. Fremont wrote that the animals were all in bad traveling order, as the grass was gone, and they were not near to any fort or trading post where corn could be procured in its place. From this cause he anticipated much delay, as his last letters prove was the case. His own health however was so entirely restored, that, with the success of his daguerreotypist, and some other causes of satisfaction, he writes in confident good spirits of his ultimate success. The next steamer from California may confirm all his hopes; for it is only reasonable to suppose his calculations may prove just, and that he will have reached San Francisco in February—about a month since.

*Extracts from letters from Bent's New Fort, Nov.* 24, 1853.

"The expedition so far, has been successful enough to gratify all our expectations, both in its general results, and in its particular object.

Our way forward now will in a few days become a struggle with the winter, forcing our way on and preserving the lives of men and animals, while at the same time we carry on the work uninterruptedly. It will amount to a wintering in the mountains, but will be a progressive one, and will fulfill all the requisitions of a winter there, as the two months of December and January exhaust the strength of the winter. * * * * Without some bad mischance, you may expect when you next hear from me, to learn of good results."

"Our condition as regards the efficiency of the camp in animals, is very bad, as regards the work we have done and are doing, very good. Provided we can reach the San Joaquin valley in the same position, our main object will be accomplished. Before reaching this place, we had lost thirteen animals. I arrived here with difficulty, traveling 10 or 12 miles a day, and having nearly all my party on foot. In the early part of the month we had encountered severe weather, the daylight thermometer being at 15° and 17°, and the buffalo had so eaten off the grass, indifferent as it was, that our animals were nearly starved. I am determined to carry this enterprise through. We will fight with the winter, and every other obstacle, to the end, prudently and cautiously, but never giving way. In the mean time, we shall do a valuable work. The astronomical, barometrical, and topographical work all goes on well. After surmounting some difficulties which required much skill to remove, the daguerreotypist has been eminently successful, and we are producing a series of pictures of exquisite beauty which will admirably illustrate the country. Every successive picture improves upon its predecessor, and those of yesterday were jewels. They were of the Cheyenne village here among the timber. As we go on, and the mountains rise before us, the views will become more interesting.

The coal formation re-appears in this neighborhood, and I have become acquainted with a locality where the coal-beds are said to be developed largely. The coal has been tried and found to burn perfectly well. It is near the line of the Road.

It will probably be two months yet before I can reach San Francisco, as I must move slowly through the mountains. Notwithstanding the early cold, we may yet have a mild and open winter. For the last 10 days the weather has been beautiful; yesterday and to-day even quite warm at mid-day—not a particle of snow to be seen, and this, as Dr. Ebers remarked, when all Germany is covered with three feet of snow."

3. *On the Action of Alkalies on Rocks;* by M. DELESSE, (communicated for this Journal by the author.)*—In order to study the action of alkalies on rocks, I have taken the powder of the rock under trial, and have heated it with a solution containing a quintuple weight of potash; I have then determined what substances were contained in the potash solution.

As a large number of rocks contain water, after I have dried the residue from the action of the potassa and determined its weight, I bring it to a red heat and take anew its weight; in this way I have obtained for different rocks the following comparative results:

* This paper should have had a place among the articles of this number.

| | I. Trachyte | II. Trachyte | III. Pertinite. | IV. Retinite. | V. Retinite. | VI. Perlite. | VII. Obsidian | VIII. Eurite. | IX. Palagonite | X. Melaphyre | XI. Basalt | XII Black lava. | XIII. Porphyry |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Silica dissolved by potash, | 36·00 | 17·06 | 19·40 | 12·23 | 9·50 | 19·55 | 18·39 | 11·45 | 7·05 | 8·50 | 7 to | 4·20 | 5·35 |
| Alumina " " | *tr.* | 2·39 | 3·75 | 1·16 | 1·25 | 1·85 | 3·78 | 1·55 | 2·10 | 2·20 | 2·80 | 2·60 | *tr.* |
| Total loss of rock after calcination of the residue. | 37·85 | 27·27 | 30·15 | 17·89 | 16·55 | 26·85 | 24·44 | 17·20 | 18·60 | 18·41 | 15·35 | 8·50 | 5·80 |

I. Brownish red Trachyte with gray globules and blackish mica, from Hungary.
II. Trachyte "molaire," with a little orthoclase and mica, from Hungary.
III. Blackish brown Pertinite, from Planitz.
IV. Globulous Retinite of a maroon-red color, from Meissen.
V. Retinite without lustre, and very resinous, from Sardinia.
VI. Grayish white and black Perlite, from Cape de Gata.
VII. Deep black Obsidian, from Lipari.
VIII. Argillaceous Eurite, of a brick-red color, from Saxony.
IX. Yellowish brown Palagonite, with a resinoid paste, from Iceland.
X. Melaphyre with a dark green paste containing greenish-white Labradorite, from Belfahy.
XI. Basalt with a blackish paste and crystals of Augite, from Bohemia.
XII. Black Vesuvian Lava, from the stream of 1819.
XIII. Quartziferous porphyry with a cellular paste of a reddish-gray color and much glassy quartz, from Saxony.

The following general conclusions may be deduced:

When a rock is attacked by an alkali, some alumina as well as silica is removed, with also some water, potash and soda; besides also a little lime, magnesia, and traces of oxyd of iron. The amount of silica removed exceeds that of all the other substances.

The alumina and water follow next after the silica in amount removed.

Granite is not thus attacked when boiled with an alkaline solution; Quartziferous Porphyry is feebly attacked, losing only some hundredths. Lava, Basalt, and Melaphyre, lose less than 20 per cent.

Trachyte, Retinite, Perlite and Obsidian, suffer the largest loss, but not exceeding 40 per cent.

A rock containing water is attacked much less readily by alkalies after it has been calcined; for the Perlite of Cape de Gata, for example, the loss before and after calcination, is nearly as $2\frac{1}{2}$ to 1.

A rock is much more easily attacked when partially decomposed. Argillaceous Eurites or Kaolins, which are only decomposed granitic rocks, experience a much greater loss than the granitic rocks.

Other things equal, the action of the alkalies is greater the larger the amount of silica, or the less crystalline their structure, and the less of hyaline quartz they contain.

The vitreous rocks, which contain little or no quartz, like Retinite, Perlite, Obsidian, Trachyte, are strongly attacked by alkalies.

On substituting alkaline carbonates for the alkalies, certain rocks, and especially the vitreous, are still attacked, but to a less extent.

The facility with which the alkalies and even the alkaline carbonates attack rocks, shows that it is difficult to use them for separating the free or immediately soluble silica which may exist in a rock, especially in clays and kaolins.

I observe, also, that in obsidian, for example, the silica which is dissolved is not *free* silica, but it is in the condition of a silicate attackable by the alkali; so also with Retinite, the silica is not in the state of opal, as generally regarded, but in that of an attackable hydrosilicate. In a

word, in all vitreous or porphyritic rocks, hydrated or not, the silica is in combination, forming a compound, not definite, which the alkali attacks, and which is the paste of the rocks.

The waters of infiltration which penetrate rocks contain always small proportions of alkaline salts, even near the earth's surface; and hence it is obvious that these salts should contribute towards the decomposition of the rocks and the formation of pseudomorphs. But at a small depth below, the waters are more largely charged with alkaline salts, and both temperature and pressure increase rapidly; they may attack, therefore, quite strongly, the rocks with which they come in contact; as happens notably with mineral waters, geysers, and other results of volcanic action. Consequently the action of alkalies or of alkaline salts on rocks plays an important part, not only in the formation of pseudomorphs, but also in the chemical reactions which take place in the interior of our planet.

4. *On the Prosopite of Scheerer;* by JAMES D. DANA.—Prosopite is described as a new mineral by Scheerer in Poggendorff's Annalen, No. 10, 1853, p. 315. The mineral occurs at the Tin mines of Altenberg in crystals altered mostly to a kaolin; and from some qualitative trials of unaltered portions, it is supposed to consist of aluminium, calcium, fluorine and water. It had been regarded as a pseudomorph after Heavy Spar: Scheerer recognizes its relations to that species in the angles, and also marks differences; Heavy Spar has the three angles 77° 43′, 128° 36′, 105° 24′, and Prosopite correspondingly 77° 30′, 132°, 116½°. Unlike Heavy Spar, the crystals are hemihedral. The faces of the crystals are dull, and admit of measurement only with the common goniometer. Scheerer suggests that the formula may be $CaF+AlF^3$, analagous to that of Heavy Spar which is $CaO+SO^3$.

This comparison with Heavy Spar does not exhibit its true affinities. In fact the angles are *almost identical with those of Datholite*, with which it also agrees in its hemihedral character. The following angles show this resemblance. The planes of Datholite here referred to will be learned from the figures on page 215 of this volume.

| *Datholite.* | *Prosopite.* |
|---|---|
| $i\breve{2} : i\breve{2} = 76° 44'$ | $i\breve{2} : i\breve{2} = 77°\text{–}78°$ $(=d : d'$ of Scheerer.) |
| $O : 2\breve{i} = 135°$ | $O : 2\breve{i} = 135°$ $(=C : B)$ |
| $2 : 2$ (adj.) $= 131° 52'$ | $2 : 2$ (adj.) $= 132°$ $(=z : z')$ |
| $2\breve{i} : 2\breve{i}$ (top) $= 115° 26'$ | $2\breve{i} : 2\breve{i}$ (top) $= 116\frac{1}{2}°$ $(=o : o')$ |
| $2\breve{3} : 2\breve{3}$ (adj.) $= 118° 9'$ | $2\breve{3} : 2\breve{3}$ (adj.) $= 119° = (t : t')$ |

Owing to the disguised character of the mineral, it was named *Prosopite* from *προσωπειον, a mask*—a name certainly well deserved.

The symbols of the planes used above become Naumann's on inserting a P, and putting $\infty$ for $\breve{i}$. The plane $i\breve{2}$ ($\infty P\breve{2}$) is that usually considered $\infty P$.

5. *Geological Survey of Tennessee.*—The State of Tennessee has ordered a Geological Survey of its territory, and appointed to the work Prof. J. M. Safford of Cumberland University, Tennessee. The appointment is a most excellent one. Professor Safford is well prepared for the duties, and his final Reports will, beyond doubt, prove both valuable and honorable to the State and to science.

6. *Telegraphic Longitude at Brussels.*—Under this title, the Athenæum of January 14th, states that the American method of determining difference of longitude by means of the Electric Telegraph, has at last been introduced into Europe. "The Royal Observatory at Greenwich is now permanently connected by one line of wires with the South Eastern and Electric Telegraph System, and by another line with the System of the Submarine and Electric Telegraph." At the observatories of Brussels and Greenwich, about 3000 signals have been observed simultaneously for the comparison of the two transit clocks. The lines are available also for experimenting on the time occupied by the passage of the galvanic pulse from Greenwich to Brussels and the reverse. And from the observations thus far made, it appears that the time is "pretty accurately *one-tenth of a second.*" "Rapid as is the velocity which this implies (about 2700 miles per second, supposing the velocity uniform along the whole line) it is much less rapid than that found in the experiments with Edinburgh (about 7600 miles per second), and still less than that determined on some of the American lines (about 18000 miles per second). The difference undoubtedly depends on the circumstance that the greater part of the line to Brussels is subterranean and submarine, which position of the wires, without in any degree impairing the insulation (which, perhaps, is the most perfect in the world), does by an ill-understood effect of induction, greatly retard the speed of transit."

It is expected that before long Greenwich will be connected in a similar way with the French and Dutch observatories, and these with others over Europe.

"Nearly the whole of Europe is now covered with a net of geodetic triangulation, connecting the western coasts of Ireland and France with the interior of Russia and borders of Turkey. The combination of the geodetic measure with the ascertained difference of longitude will afford one of the best materials for the measure of the earth."

7. *The World of Science, Art and Industry, illustrated from Examples in the New York Exhibition*, 1853 '54. Edited by Prof. B. Silliman, Jr., and C. R. Goodrich, Esq., aided by several scientific and literary men. With 500 illustrations, under the superintendence of C. E. Döpler, Esq. 200 pp. 4to., New York, 1854, G. P. Putnam.—This work, independently of the great number and elegance of its illustrations, has a high value as a record of the Exhibition at New York in 1853, and also on account of the many essays it contains on subjects connected with the recent progress of practical science and the various arts.

8. *The Electro-Magnetic Telegraph;* by Laurence Turnbull, M.D., 2nd edition, revised and improved, illustrated by numerous engravings. 264 pp. 8vo. Philadelphia, 1853. A. Hart.—This work on the Telegraph is practical, scientific and historical, and the best exposition we have of the American system. Moreover, in an appendix it contains several important telegraphic decisions and laws. The work would be improved by an account of the application of the Telegraph to the determination of longitude.

9. *Outlines of a Mechanical Theory of Storms*, containing the true [?] law of lunar influence, with practical instructions to the navigator, to enable him approximately to calculate the coming changes of the wind and weather, for any given day and for any part of the ocean; by T. BARNETT. 246 pp. 12mo. New York, 1854. D. Appleton & Co.

10. *Fownes' Chemistry for Students.*—A new edition of this well established and valued work has just been published by Messrs. Blanchard & Lea, of Philadelphia, from the last Lond. edition by Drs. Bence, Jones and Hoffman. The American edition is edited with care by Dr. Bridges.

11. *A Manual of British Mineralogy;* by R. P. GREG, F.G.S., and W. G. LETTSOM. 8vo. Price £1 1*s.*—The British Mineralogy of Messrs. Greg & Lettsom is now in press and its appearance is promised in the course of the season. The work, as we learn, and should infer from our knowledge of the authors, will be a thorough treatise on the minerals of Britain, and will contain a large amount of original matter, descriptions and figures of many new forms of crystals, new analyses, besides statistical and other useful information respecting the mines of Britain and their products. The work will be illustrated by nearly 300 wood-cuts and a few colored lithographic plates, illustrating some unique and remarkable specimens.

12. *My Schools and My Schoolmasters; or, the Story of My Education*, by HUGH MILLER. Edinb., Johnstone & Hunter. (Extracts from a notice in the Athenæum of March 11, 1854).—Mr. Miller's grandfather was a buccaneer—his father was a sailor—to whom he was born, the first child of marriage, in the remote shire of Cromarty, A. D., 1802. The boy showed some glimpses of the fierce and piratical thews and sinews of his ancestor in his early days, when he laid the hands of force on his schoolmaster, who tried to flog him because he would not spell "*awful*" in the right way; but earlier even than this, he had become a devourer of books—(and to that remote place the 'Arabian Nights' and 'Gulliver,' and 'Captain Cook's Voyages' and 'Udolpho,' and 'Ambrose on Angels,' penetrated as well as 'the Shorter Catechism,' the Proverbs and the New Testament)—and an observer of the minute aspects and hidden things of nature. He had begun to watch the habits of birds, and to note the colors of flowers, and to try to read that which (to avail ourselves of one of Mr. Ruskin's fantastic figures of speech) creative wisdom has written in the caverns of the earth. His kith and kin, some of them pure Highlanders, were men of marked character,—so that, with these surroundings, and these propensities of mind and endowments of body, there was small chance of the Cromarty boy lacking such adventures as serve a bold spirit for schooling, and an apt intelligence instead of schoolmasters. * * *

Active life, however, must needs be commenced, and as the boy was not particularly amenable (as may have been guessed) to academical discipline, and as his kinsfolk had little choice within their reach, there was no alternative for him save to betake himself to manual labor. Regarding this again, he shall tell his own story.

"The husband of one of my maternal aunts was a mason, who, contracting for jobs on a small scale, usually kept an apprentice or two, and employed a few journeyman. With him I agreed to serve for the term of three years; and, getting a suit of strong moleskin clothes,

and a pair of heavy hob-nailed shoes, I waited only for the breaking up of the winter frosts to begin work in the Cromarty quarries—jobbing masters in the north of Scotland usually combining the profession of the quarrier with that of the mason. * * The quarry in which I commenced my life of labor was, as I have said, a sandstone one, and exhibited in the section of the furze-covered bank which it presented, a bar of deep-red stone beneath, and a bar of pale-red clay above. Both deposits belonged to formations equally unknown at the time to the geologist. The deep-red stone formed part of an upper member of the Lower Old Red Sandstone; the pale-red clay, which was much roughened by rounded pebbles, and much cracked and fissured by the recent frosts, was a bed of the boulder clay. Save for the wholesome restraint that confined me for day after day to this spot, I would perhaps have paid little attention to either. * * *

Though now seventeen, I was still seven inches short of my ultimate stature, and my frame, cast more at that time in the mould of my mother, than in that of the robust sailor, whose 'back,' according to the description of one of his comrades, 'no one had ever put to the ground,' was slim and loosely knit, and I used to suffer much from wandering pains in the joints, and an oppressive feeling about the chest, as if crushed by some great weight. I became subject, too, to frequent fits of extreme depression of spirits, which took almost the form of a walking sleep—results, I believe, of excessive fatigue—and during which my absence of mind was so extreme that I lacked the ability of protecting myself against accident, in cases the most simple and ordinary."

The reader is referred to the work itself for the continuation of the eventful and instructive history, the final result of which, in the able geologist, Hugh Miller, is familiar to all.

13. *Annual of Scientific Discovery, or Year Book in Science and Art for* 1854—exhibiting the most important discoveries and improvements in Mechanics, Useful Arts, Natural Philosophy, Chemistry, &c., edited by David A. Wells, A.M. Boston, 1854. Gould & Lincoln.

14. *Smithsonian Contributions to Knowledge*, vol. v, 1853.—Art. I. Introduction.

II. A Flora and Fauna within living animals; by Joseph Leidy, M. D.

III. Memoir upon the Extinct Species of Fossil Ox; by J. Leidy.

IV. Anatomy of the Nervous System of the Rana pipiens; J. Wyman.

V. Nerealis Boreali-Americana, Part II; by Wm. Henry Harvey.

VI. Plantæ Wrightianæ Neo-Mexicanæ, Part II; by Asa Gray.

15. *Seventh Annual Report of the Board* of Regents of the Smithsonian Institution for the year 1852. Washington, 1853.

Wm. Stimpson: Synopsis of the Marine Invertebrata of Grand Manan, or the region about the mouth of the Bay of Fundy, New Brunswick—From the Smithsonian Contributions to Knowledge. 56 pp. 4to, with two plates. Contains descriptions of many new species and some new genera in the different orders of Invertebrata.

Dr. J. Victor Carus: System der thierischen Morphologie, 506 pp. 8vo, with 97 wood-cuts. Leipzig, 1853. W. Engelmann.

Hanns Bruno Geinitz: Die Versteinerungen der Grauwackenformation in Sachsen und den angrenzenden Länder-abtheilungen. Heft II. 4to, with 20 lithographic plates. Leipzig, 1853. W. Engelmann.

—This work and the preceding were received too late for a further notice in this number.

PALÆONTOGRAPHICAL SOCIETY: Volume for 1853—rich in its contents, containing Part IV of Fossil Corals of Great Britain; Part IV of Fossil Brachiopoda of Great Britain; Part I of Fossil Shells of the Chalk; Cephalopoda, by Daniel Sharpe, F.R.G. & L.S; Part II of a Monograph of the Mollusca from the great Oolite, chiefly from Minchinhampton, and the coast of Yorkshire, by J. Morris and John Lycett; Part III of the Crag Mollusca, by Searles V. Wood; Part IV of the Fossil Reptilia of Great Britain, by Richard Owen. Illustrated by numerous plates. The annual subscription to the Palæontographical Society is one guinea.

E. SABINE: Observations made at the Magnetical and Meteorological Observatory at Hobarton, in Van Diemen Land, printed by the order of Her Majesty's Government, under the superintendence of Col. Edward Sabine, of the Royal Artillery. Vol. III, commencing with 1846. 622 pp. 4to. London, 1853.

*American Association for the Advancement of Science.*—The next meeting of the Association will be held in Washington, D. C., commencing with Wednesday the *twenty-sixth* of April.

F. OVERMANN: Practical Mineralogy, Assaying and Mining, etc., 2nd Edition. 230 pp. 18mo. *Philadelphia.* Lindsay & Blakiston, 1853.

PERUVIAN ANTIQUITIES, by Mariano Edward Rivero, and John James von Tschudi; translated into English by Francis L. Hawks, D.D., LL.D. *New York*, 1853. G. P. Putnam. A work of great popular and scientific interest.

JAMES D. FORBES: Norway and its Glaciers visited in 1851, followed by Journals of Excursions in the High Alps of Dauphiné, Berne, and Savoy. *Edinburgh*, 1853.

ADOLPH SCHLAGINTWEIT: Über die orographische und geologische Structur der Gruppe des Monte-Rosa. 20 pp. 4to, with 2 tables. *Leipzig*, 1853. Also, Neue Untersuchungen über die physicalische Geographie und die Geologie der Alpen, by Adolph Schlagintweit und Herm. Schlagintweit.

PROCEEDINGS OF THE ACAD. NAT. SCI. PHILADELPHIA, vol. vii, No. 1.—p. 3. On a new Entomostracan of the family Limnadidæ (n. g. Limnadella); *C. Girard.*—p. 4. On the Ancient Alluvium of the Ohio River and its Tributaries; *A. T. King.*—p. 8. Notice of American Animals formerly known but now forgotten or lost; *John Le Conte.*—p. 16. Descriptions of some new Coleoptera from Oregon; *J. L. Le Conte.*—p. 20. Synopsis of the Œdemeridæ of the United States; *J. L. Le Conte.*

ANNALS OF SCIENCE, Cleveland, Ohio. JANUARY.—p. 2. New species of fossil plants; *J. S. Newberry.*—p. 4. Poecilosoma erythrogastrum, a fish from Rocky river near Cleveland; *J. P. Kirtland.*—p. 5. List of diurnal Lepidoptera of Northern and Middle Ohio; *J. P. Kirtland.*—p. 6. New locality of the Limnea megasoma of Say; *J. P. Kirtland.* FEBRUARY.—p. 44. On the viviparous or ovoviviparous character of a species of the Salmonidæ: *T. Garlick.*—p. 44. Alburnus nitidus; *J. P. Kirtland.*—p. 45. Observations on diurnal Lepidoptera of Northern and Middle Ohio; *J. P. Kirtland.* MARCH.—p. 71. On the artificial reproduction of Fishes; *T. Garlick.*—On the diurnal Lepidoptera of Northern and Middle Ohio; *J. P. Kirtland.*—p. 77. Analyses of Ohio coals; *J. S. Newberry.*—p. 78. Revision of the species of the genus Esox, inhabiting Lake Erie and the River Ohio; *J. P. Kirtland.* APRIL.—p. 100. On Alburnus acutus; *I. A. Lapham.*

Received from the Publishers:

GUSTAV LENZ: Ueber die geschichtliche Enstehung des Rechts. Eine Kritik der historisch Schule. 352 pp. 8vo. *Greisswald* and *Leipzig*, 1854. C. A. Koch's Verlags Buchhandlung, Theodor Kunike.

A. H. BAIER: Symbolik der christlichen Confessionen und Religions-partheien. Erster Band. Symbolik der römisch-katholischen Kirche. Erste Abtheilung, Die Idee und die Principien des römischen Katholicismus. *Greisswald* and *Leipzig*, 1854. C. A. Koch's Verlags Buchhandlung, T. Kunike.

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